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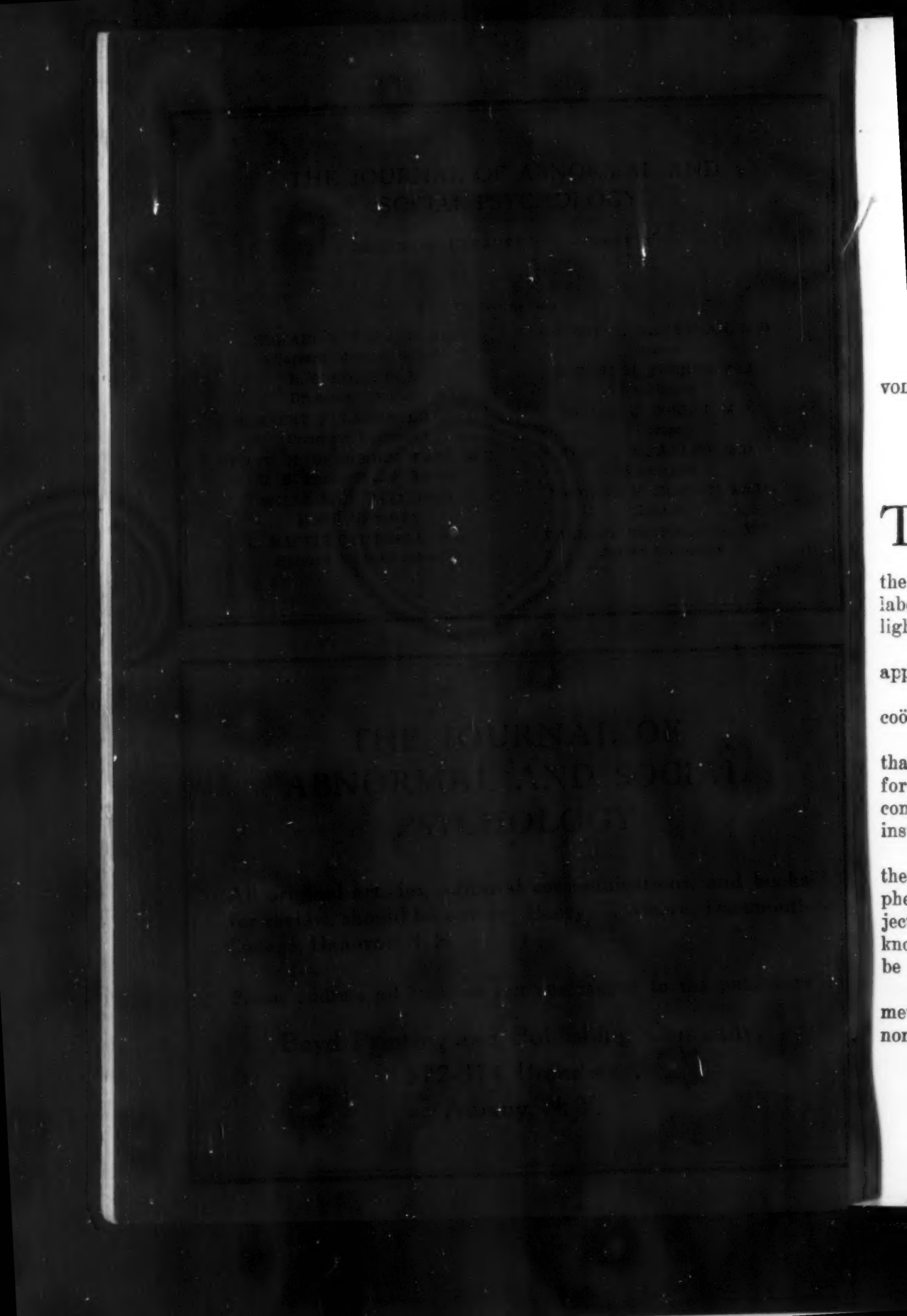

THE JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

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EDITORIALS

$5,000 AWARD

THE JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY offers an award of $5,000 to any person claiming to produce supernormal material phenomena who will be the first to demonstrate the actuality of the same under rigid laboratory conditions and by recognized scientific methods, in full light, under the following further conditions:

1. The investigations will be carried out by a committee appointed by the JOURNAL.

2. The medium must freely, voluntarily and wholeheartedly coöperate with the committee.

3. To gain the award it must be demonstrated to the committee that the phenomena are bona fide, supernormal, and due to some force not previously known to science. They shall not be due to conscious or subconscious behavior of the medium such, for instance, as trickery of any kind.

4. The phenomena must be subject to instrumental control and therefore, whatsoever their cause, must be material or physical phenomena; they must be visible, produced in full light and subjected to instrumental recording, measuring and testing. Any known physical or chemical means of testing the phenomena must be permitted to be used.

In short, the conditions include any and all scientific laboratory methods such as would be used in investigating physical phenomena. Psychic photographs are not included as phenomena.

Of course, the committee will not do anything without the full,

free consent of the medium or do anything that will intentionally injure in any way the medium, or anything contrary to social or scientific ethics; but if any method considered essential for or used in scientific research is refused by the medium the right is reserved to terminate the investigation at the will of the majority of the committee without recourse or right of claim by the medium to the award.

5. While the primary object of the award is to determine only the actuality of alleged supernormal phenomena, the medium must coöperate with the committee in discovering the cause of the same, if the committee desires to go further and make such investigations as may be necessary.

6. Entries must be made to Dr. S. Burt Wolbach, Professor of Pathological Anatomy, Harvard Medical School, Boston, secretary to the committee, and will close November 1, 1925. To avoid wasting the time of the committee any medium wishing to compete must present in advance evidence satisfactory to the committee that he or she has produced phenomena worthy of investigation. Entries will be listed in the order received, but the right is reserved to determine the order in which they shall be investigated. The award will be made only to the first medium who produces and proves to the satisfaction of the committee a supernormal phenomenon (in the meaning of this offer) of such sort that a permanent physical record can be made of its occurrence.

7. The award must be made by a unanimous vote of the committee. The investigations will be made in a laboratory or other place in the neighborhood of Boston at the discretion of the committee.

8. The committee reserves the right to invite to its assistance and to admit to or exclude from its investigations of the medium any person as it sees fit.

9. The refusal of a medium to conform to any condition imposed by the committee will be sufficient cause to terminate the investigation and the medium will cease to be an applicant.

10. The following have consented to serve on the committee:

Professor Harlow Shapley, Paine Professor of Practical Astronomy and Director of the Harvard Observatory.

Professor Theodore Lyman, Professor of Physics and Director of Jefferson Physical Laboratory, Harvard University.

Dr. Walter B. Cannon, Professor of Physiology, Harvard Medical School.

Dr. C. C. Pratt, Instructor in Psychology, Harvard University.

Mr. Harry Houdini.

In case of a vacancy due to any cause, the committee will appoint a competent expert to fill the same. The committee may, also, add to its members.

All communications should be addressed to the Secretary, Professor S. Burt Wolbach (address above).

The JOURNAL is led to offer this award by the following considerations:

As the writer pointed out in a communication to the *Boston Herald,* investigations of so-called supernormal phenomena claimed by mediums are well worth pursuing if carried out under scientific experimental conditions. It must be conceded that they may possibly turn out to be of the utmost importance for science; which means for gathering data to give us a deeper knowledge of the universe. If it could be demonstrated that the phenomena in question, putting aside the spiritualistic interpretation, are bona fide and can be correctly interpreted as due to forces which are manifestations of that same energy of the universe which is manifested as electricity, electromagnetic phenomena, light, heat, ultra-violet and infra-red rays, radio activity, X-rays, Hertzian rays, chemical activity, etc., it would be one of the most important discoveries that could be made and would rank with the modern discovery of the electrical nature of matter. But such manifestations would not be supernormal though the discovery might well be worthy of a Nobel prize.

On the other hand, if it could be demonstrated that the alleged phenomena could be interpreted as due only to some hitherto undiscovered "force" other than that recognized by physical science, as is generally claimed by mediums, they would be classed as "supernormal" as that term is commonly understood. If either of these theories could be substantiated we should be able, and even compelled, to formulate a new and broader conception of the ultimate nature of energy and of the universe and even of mind. But, of course, preliminary to any interpretation, the claimed phenomena in question must be shown to be bona fide and

not due to conscious or unconscious deception, which has not yet been done and probably cannot be done under the conditions imposed on the investigators.

The labor of the *Scientific American* committee, so far as the investigation just completed of the medium "Margery" is concerned, has ended as it was bound to end, owing to the hampering conditions under which they were compelled to work, in the Scotch verdict of "not proven" by a vote of 4 to 1 of the committee. Thus the question of the veridity of supernormal phenomena in principle has been left just where it was. This was all, of course, the committee undertook to do, or could be expected to do under the restricted conditions and arbitrary rules imposed by the medium by which "all the cards in the game are stacked in favor of the 'medium' ". Accordingly the committee reported:

> "We have observed phenomena, the method of production of which we cannot in every case claim to have discovered. But we have observed no phenomena of which we can assert that they could not have been produced by normal means, although we have looked for such phenomena patiently and with open minds. It is obvious that we cannot prove that the 'medium' never has produced and never can nor will produce supernormal phenomena. But in our opinion we have afforded the 'medium' ample opportunities for the demonstration to us of such phenomena and no such demonstration has hitherto been made.
>
> "Therefore, we report that, in our judgment, the 'medium' is not entitled to the award of the prize offered by the *Scientific American.*"

It is obvious that no investigations (which, after all, are really of the nature of experiments) that will conclusively demonstrate the actuality of "supernormal" phenomena and still more, if proved, their nature and source, can possibly be carried out excepting under strictly scientific laboratory methods permitting of rigid control, not only of the medium, but of the experimenters themselves. One of the essential conditions for control is a full, adequate light. Another requirement is that the experimenters shall alter, modify and determine the conditions at will, according to the effects produced, etc., and use any essential method known to science for the examination of the phenomena (including the so-called "ectoplasm").

It is safe to say that no conclusions reached, whether favorable or unfavorable, positive or negative, drawn from researches made, as in the case of "Margery", in a dark room, or in such dull red light as to require the use of luminous paint on objects to make them visible, will ever be accepted as final by a consensus of opinion. Those with the will to believe, will believe; those with the will to disbelieve, will disbelieve; those of a critical, scientifically trained mind will remain agnostics. It will be a question of belief not of proof.

Nor does it prove anything that the same phenomena can be produced by trickery, nor even that the medium has been detected in deceit. As every one knows, or ought to know, a person in a trance and, for that matter, when fully awake, will subconsciously and without being aware of the fact, practice fraudulent acts. This does not militate against the conscious probity of the medium. We have no doubt in our own mind, having observed and studied subconsciously produced phenomena for many years, that much of so-called table-tipping, planchette writing, etc., is done in this way, without conscious deceit. And the same is true of much of the other type of phenomena known as subjective communications revealing alleged supernormal knowledge. It is for this reason that the experimenters themselves as well as the "medium" must be under controlled observation. In darkness this is impossible.

It, of course, will be said, in fact often is said, that "supernormal" materialistic phenomena cannot be produced in bright light and this statement is made to appear plausible by the concocted theory that light waves interfere with the alleged psychical or spiritual, or other force emanating from the medium, just as radio waves of the "ether" are interfered with by other waves and broken up. Well, the answers to that are three:

First, it needs to be demonstrated by exact methods of experimentation. Second, if true, that ends all adequate, reliable investigation. Third, it isn't true in principle, though it might be true in a particular case.

The celebrated medium, D. D. Home, who produced most remarkable materializations in the latter part of the last century and who was investigated by the eminent scientist, Sir William Crookes (of Crookes tubes fame), produced his phenomena "at all times and seasons, under all sorts of conditions—in broad daylight, in artificial light, in semi-darkness, at regular séances, unpremeditatively without any séances at all, indoors, out of

doors, in private houses, in hotels—at home and abroad". So it is stated in the recent report of his experience with Home by the Earl of Dunraven, who had "an intimate friendship with him and spent a good deal of time during two years in his company" observing the phenomena. Even Eusapia Palladino, though discredited because of having been detected in fraud, caused (according to the eminent Professor Richet) a table to rise in the air in the full blaze of sunshine with no one touching it. (Of course, we are not vouching for these alleged facts.)

The average layman does not realize how rigid is the control required by scientific methods in all laboratory research if any conclusions reached are to be accepted. And even then, such are the possible errors of observation and method, almost all, if not all, observations must be checked up and confirmed by independent observers before final acceptance.

It is with these various considerations among others in mind and with the hope of stimulating the search for truth and bringing alleged supernormal phenomena within the domain of scientific research that this JOURNAL offers the above award.

ANNOUNCEMENT

We take pleasure in announcing that beginning with this number, Professor Henry T. Moore, of the Psychological Department of Dartmouth College, has become associated with our Editorial Staff and will coöperate in the active work of editing the JOURNAL. Dr. Moore, who takes the place of Dr. Floyd H. Allport (whose resignation we regretfully announced with our last issue) will particularly devote his activities to the department of social psychology, a field in which he is a leading authority.

Our readers will also be glad to learn that the staff of associate editors has been strengthened by the addition of Dr. James Drever, of the University of Edinburgh; Dr. Franklin H. Giddings, of Columbia University; Dr. Floyd H. Allport, Syracuse University, and Dr. Henry Herbert Goddard, of the Ohio State University, whose contributions to our editorial columns we feel sure will be welcomed.

All original articles, editorial communications and books for review should be addressed to Professor Moore, Dartmouth College, Hanover, N. H.

INTELLIGENCE TESTING AND PERSONALITY RATING FOR VOCATIONAL GUIDANCE IN COLLEGES

NO ONE who has been in intimate contact with the work of American colleges can have failed to recognize the desire and even need on the part of many students for some sort of guidance in the solution of their personal problems. Not all will agree, however, on the kind of guidance best adapted to undergraduate needs and desires. This JOURNAL has already taken notice editorially of the systematic attempts which are being made in some quarters to bring to bear upon the problem of undergraduate guidance all the refinements of psychological technique employed in the analysis and rating of intelligence, personality, and various special abilities. At the conference on vocational guidance held in Washington last year by the Committee on College Entrance Tests of the National Research Council, reports were made and views expressed on the advisability and possibility of extending the methods of intelligence testing and personality rating to the problems of vocational guidance in colleges, and it was agreed that colleges would be in a position to render much more effective service to students by developing the machinery for collecting and utilizing the data which these methods yield. The idea is a meritorious one and will undoubtedly win the support of all but the most conservative educators as soon as it becomes more generally apparent that each new generation of college students faces the necessity of making adjustments and adaptations to an increasingly intricate and vexatious social environment.

But the program which such a scheme of systematic rating of psychological traits calls for is an ambitious one and as yet beyond the means which many colleges would feel able or justified in setting aside for it. There are other difficulties. Most psychologists themselves will admit that their science is only on the threshold of the study and analysis of intelligence, personality, emotional traits, and the deep-seated springs and drives of human action. And intellectual aptitudes, character, emotion, and drives, as presented to the psychologist in the form and activities of young college students in the classroom, are, or at any rate should be, peculiarly difficult to examine by those methods which may be employed with some degree of success when these same psychological factors are isolated and represented in terms of some more or less artificially controlled quantitative output. It is possible to argue that in allowing psychological technique to subserve

certain premature technological demands psychologists have put the cart before the horse. Quantitative determinations which have not been preceded by qualitative analyses are of dubious scientific value.

With such reservations in mind with respect to his program for taking mental measurements of college students, the psychologist will find himself embarrassed in his efforts to break down academic conservatism. He will be still further embarrassed by those educators who will tell him that the scheme which he proposes is not one which the ideal college wants or is supposed to have much to do with; that the purpose of the liberal college is not to fit men to practical jobs of life nor to reduce the number of square pegs in round holes, but to create within the student a familiarity with and permanent interest in the achievements and values of science and art. It is intended that the acquisition of this knowledge by the student should enable him to appreciate in as large measure as possible the fine art of living rather than to control and secure with efficiency the means of livelihood; whereas the efforts of much recent psychological measurement have been directed to the rating of capacities which are sought after in the world of practical affairs. One may have good reason for doubting whether a positive correlation exists between these capacities and the traits which a college education should help a man to cultivate.

But there are indications here and there of certain changes in American educational attitudes and machinery which may make it possible for the psychologist to play an important part in the guidance of college students before he has perfected his rating scales and had them accepted by college institutions. The present agitation against the rigidity and formality of the lecture system and against the absence of personal contact between instructor and student is likely to result in the more general adoption of measures, similar to those in vogue at Harvard and Princeton, which are designed to treat students as something more than factory products to be turned out on a large scale in accordance with approved and well ordered methods of production. If this change means that American college administrators are beginning to realize that it is a mistake to forget that the ideal college is represented by Mark Hopkins on one end of a log and a student on the other, then it may come about that the psychologist will be in a position to contribute valuable suggestions regarding the sort of personal interview which could take place occasionally between

instructor and student. The situation created by an intimate and frank conversation between two people is one highly favorable to the psychologist who would understand the doubts, anxieties, conflicts, fears, and what not, of an individual. And it is such states of mind that a student often desires to talk about frankly with someone in whom he has confidence.

If those members of a faculty who come into personal contact with students could meet from time to time to discuss with a psychologist the way in which certain traits of human character are often revealed in conversation, a great deal could be done by these instructors, in spite of their lack of systematic psychological training, to aid the student to know himself. The function of an instructor who acts towards his students *in loco parentis* could be made exceedingly effective by using what general principles a psychologist could give him regarding the operation of such mental mechanisms as repression, conflict, rationalization, etc. A knowledge of such general principles combined with his own natural insight into human nature would enable him to deal sympathetically and intelligently with those students who feel the desire to discuss their personal problems with an older individual. And the instructor could send students whose cases seemed peculiarly baffling and complex to the psychologist for special study. Such methods would support and advance tremendously the effort, which bids fair to become increasingly more general in institutions of higher learning, to break down the barrier separating instructor and student, and would assist the college in fulfilling what some would consider its primary purpose, namely, to give to a student the opportunity for a wider and deeper knowledge of self, and a clearer recognition and richer cultivation of his permanent interests.

CORRESPONDENCE

With this number of the JOURNAL we open our pages to correspondence and invite our readers to address letters to the Editor on topics of lively and general interest. In our view there are constantly arising many such topics which suggest comment or discussion and to which a correspondent would like informally to call attention through a letter but which are not of such scientific importance as to invite a formal article. With this idea in view we shall be glad to publish a limited number of letters to the Editor and invite such correspondence.

IS THE CONDITIONED REFLEX THE UNIT OF HABIT?

By GEORGE HUMPHREY
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"HABIT," says Watson, "is a series of conditioned reflexes. The conditioned reflexes are the units into which all habits may be resolved."[1] This statement is often made, and more often assumed. It is the object of this paper to examine it and its implications. In what follows, by conditioned reflex is understood the mechanism investigated in Pavlov's laboratory, where the most fundamental and most carefully controlled experiments were conducted.

If a comparison is made between Pavlov's conditioned reflex and the habit of everyday life, certain marked differences appear. I quote from Madame Dontchef-Dezeuze.[2]

"A peculiarity . . . is the extreme instability of this reflex. This instability shows itself in various forms, which are: extinction, inhibition, and revival. All the writers have spoken of these different forms of instability." Of these only the question of inhibition need concern us here. "From the moment," says the same writer, "that it is not supported by the unconditional reflex to which its origin can be traced it gradually shrinks and ends by disappearing." This is not characteristic of the habit as we ordinarily know it. If it were, it would mean that we should never be able to write without the occasional presence of the original motive for writing, nor to read intelligently without a temporary coincidence, from time to time, of at least a picture of the object with the printed word. Of course, we always have a motive for reading, or we should lose the habit. But Pavlov's whole point is that the reaction, to be retained, must be supported by the original exciting stimulus. On this count alone, if a habit is made up of conditioned reflex units, it is difficult to see how we can really acquire a habit. Furthermore, the conditioned reflex, in the strict sense of the term, is inhibited with the most astonishing ease. The passage in question is so striking that I cannot refrain

[1] Quoted by Burnham, *The Normal Mind*, page 179. The writer has been guilty of the same language. See *The Conditioned Reflex and the Freudian Wish*. JOURNAL OF ABNORMAL PSYCHOLOGY, Feb. 1920.

[2] L'Image et les Réflexes Conditionnels dans les travaux de Pavlov. p. 68.

from quoting it in full. "Constantly, every minute, one might say, during our experiments there may be seen a positive reaction of the animal . . . to each variation of the surrounding situation. Every sound, however faint, that chances to occur amid the constant sounds and noises that surround the animal, every weakening or increase of these constant noises, every oscillation of the intensity of the general illumination of the room, whether the sun hides behind the clouds, or a ray of light crosses them, every sudden raising or lowering of the clearness of a little electric lamp, a shadow which crosses the window or the room, a fresh odor that spreads in the room, a puff of cold or warm air that penetrates it, even the lightest possible grazing of the animal's skin, such as a fly, a fragment of plaster from the cornice of the ceiling, etc."[3] may excite the activity of this or that part of the animal and inhibit the reflex. Pavlov goes on to say that these adventitious disturbances, if repeated, rapidly become indifferent, but that they can never be eliminated entirely, because they are innumerable, and because they reëstablish themselves if the repetition is intermittent. Other similar causes for inhibition are given, such as overfullness of the bladder, a slight lesion of the mouth, etc. Here again one must conclude that we are not dealing with habit, in the ordinary sense of the word. The underlying drive with which Pavlov was dealing was the powerful hunger motive, and the habits which the animals actually establish round this motive in their natural life are certainly not thus hypersusceptible to outside disturbance. If the habits of the animal can be analyzed into conditional reflexes of the Pavlov type, then it is hard to see how a cat ever answers its name.

The objection occurs that Pavlov was dealing with habits much less firmly established than the more stable ones of the animal's everyday life. But the animals were, as a matter of fact, often trained over long periods of time, and Pavlov speaks of a very lengthy series of experiments being spoiled by the cropping up of one or other of the types of inhibition described. It was, in fact, to avoid just this trouble that his extraordinarily elaborate new laboratory was designed. The objection also occurs that habits do show the characteristics of Pavlov's mechanism, but in lesser degree. But though it is true that habit is subject to inhibition of the same kind as Pavlov found, still there remains a difference of

[3] L'Inhibition des Réflexes Conditionnels, Journal de Psychologie, Vol. 10, 1913, pp. 1–15.

degree in the properties of the so-called unit of habit and habit itself which is so enormous that it constitutes a fundamental, qualitative discrepancy. One who reads the account given by Pavlov and his workers is forced to the conclusion that the mechanism described is qualitatively different from that which, say, James treated in his chapter on habit. The conditioned reflex does not seem to satisfy the condition for a natural unit that it should retain all the fundamental properties of that of which it is a unit. It lacks the relatively high stability and robustness of the habit of everyday life.

When one comes to the explanation of this loss of stability and robustness, one is on more difficult ground. The view put forward is a tentative one only, but one for which there seems to be some experimental evidence.

In his *Psychology,* Watson makes the following statement. "In the psychological laboratory, when we are dealing with relatively simple factors . . . and are attempting to isolate their effects . . . we speak of stimuli. A situation is, of course, upon final analysis, analyzable into a complex group of stimuli." From the standpoint of physical science this analysis is justified, and in fact is a presupposition of that science. But whether the *psychological situation* can thus be analyzed is at least open to question. In general, Watson seems to assume the discreteness of the psychological situation. "Stimuli," he says for example, "must be added to or subtracted from."[4] This may be simply an example of the incautious use of language, but it is exactly such incautious language that the present paper is designed to combat, by showing its unfortunate implications in the case of the conditioned reflex. There is, of course, evidence that the psychological analysis of a situation and the analysis which is most convenient for the purposes of physics or of everyday life do not necessarily lead to the same result. Thus Head, in dealing with dermal stimuli, finds it necessary to remind his readers that "the stimulus is always an external object, which evokes a multitude of physiological reactions. . . . From the physiological point of view a simple stimulus is an abstraction and an external object is a group of functional events."[5] Here the contrast between the physical and the psychological analysis of the situation is well brought out, always remembering that we mean by physical

[4] *Psychology,* page 8.
[5] Studies in Neurology, p. 746.

analysis the kind of thing of which Watson speaks in the passage first quoted. Passing from the simple stimulus to the whole situation, we have the work of Köhler on apes, which proved that a psychological situation involving a light gray and a dark gray could not be analyzed into its physical components, which are two separate areas emitting two different kinds of light wave. What is a satisfactory principle of analysis for the purposes of physical science will not serve the purposes of psychological science, even though we define psychology in terms of behavior. Investigators of the Gestalt school have shown the same thing to be true of other situations, notably Wertheimer in his classical phi-phenomenon experiments, which were later followed up by Koffka and his pupils. Furthermore, the fact of redintegration, whereby one part of the situation comes to function for the whole, a fact which has been noted by thinkers from Augustine down, seems to point to the existence of some kind of functional unity in the psychological situation which can be seen not to be of an additive nature if one thinks in terms of response rather than of cognition. The existence of such redintegration seems to have been proved by the results of experiments on the activity of reading, which is one of the few cases where the situation to which an organism reacts has been adequately analyzed from the psychological point of view. We read by clues, which come to function for the whole word, or even sentence. And again, the word, from the psychological point of view, cannot be built up out of the separate letters, nor the sentence out of the separate words. Witasek [8a] showed the same to be true of the memorization of series of nonsense syllables, which appear to function as non-summative wholes, and memorization is even hindered by the presence of discrete associations between the members of the series. The reading activity offers an excellent example of the distinction which is here being drawn between the psychological analysis of a situation and the crude analysis to which the term " physical " has been applied for want of a better. A line of print seems so obviously analyzable into its component letters that one has some difficulty in persuading a layman that for pedagogical purposes such an analysis is spurious and actually harmful. The same example illustrates the point that even though one were able to describe in entirely adequate terms the organism and its integrations of structure, consciousness and conduct, yet

[8a] Zeit. fr. Psych. 79, 1918.

there would still be something lacking to a complete psychological description, namely an *ad hoc* analysis of the situation to which the organism is reacting.[5b] A thoroughgoing psychology should examine every psychological situation for any natural articulations it may possess, just as it examines behavior and consciousness for their natural articulations. In order to free myself from any suspicion of metaphysical heresy, let me put the point in this way, that, whatever the ultimate nature of the "external world which stimulates the organism," when we are dealing with it psychologically, we should find it convenient to assume certain articulations which are not those of the other sciences, each of which analyzes the same "situation" differently, and in the way which best suits the phenomena it is trying to describe. For isolated cases, such as reading, this psychological analysis of the situation has been effected, but the general principle seems not to have received consideration.

Now it is clear that to assume that the Pavlovian conditioned reflex is the unit of habit is to postulate an analysis of the psychological situation which is crude in the extreme, and which is in the last degree unpsychological. It assumes, in fact, that the psychological situation can be broken up into the sum of a number of isolated, single stimuli of the kind that Pavlov used in the laboratory, single tones and noises, simple shapes, small lighted areas, and the like. Such psychology corresponds to the letter-by-letter theory of reading of the older generation of school ma'ams. The purpose of this paper is to protest against this crude procedure, and to suggest that such spurious analysis is entirely adequate to account for the difference between Pavlov's results and the results of extralaboratory experience. By careful training we may indeed bring about in an organism what is to all intents and purposes a reaction to such a spuriously isolated stimulus, and this is what Pavlov did. But such a reaction is of feeble calibre, easily breaking down and easily brushed aside. It shows, in fact, all the signs of the laboratory artefact. It is interesting that when an attempt was made in Pavlov's laboratory to build up a compound stimulus or situation, the immediate result was to inhibit the response already obtained. When a complex stimulus was obtained, such as a musical chord, that would set off the response, Pavlov describes how one note caused

[5b] The one implies the other. Both are necessary for an adequate description.

a smaller secretion than two or three notes played together, thus exhibiting an additive and not the redintegrative effect characteristic of the ordinary situation. This general rule that the stimuli he used acted in combination, to adopt his phrase, to produce a greater effect than any separate stimulus, seems to be accepted by Pavlov in his *Lancet* paper and is confirmed by the work on musical timbre, but it is contradicted by Zéliony,[6] one of Pavlov's pupils, who has denied other findings from the laboratory of his master. It should therefore, perhaps, lie under suspicion until verified, one way or the other. On the other hand, there seems indubitable evidence that such an activity as maze running cannot be analyzed into separate reactions to single stimuli. There is evidence that the rat proceeds, to quote Professor Woodworth, "not step by step, but by *runs* of some length",[7] and it is the essence of these runs that they cannot be analyzed into disconnected reactions, but must be regarded as units. The point is made also by Peterson and others. Experimental results show that we cannot think of the activity of maze running as the sum of a number of " piecemeal processes ", to quote Peterson. Here, of course, the element of succession in time is involved, but the results none the less work against the notion that the psychological situation can be analyzed into stimuli of the order of those with which Pavlov worked. For a psychological datum, psychological analysis is required, and this is not afforded by the procedure that breaks up such complex reactions into a number of discrete conditioned response units.

The criticism that is here offered of the conception of the conditioned reflex as the unit of habit can thus then be summarized. The conditioned reflex differs from habit, as the term is generally used, in that it lacks the relative robustness and self-subsistence of the latter, the discrepancy being probably due to a nonpsychological analysis of the psychological situation. The paper is in general a plea for the independence of psychology, and for the necessity of psychological treatment of psychological data. By escaping into the relative objectivity of behaviorism the science has not, as seemed at first the case, left behind the problem in analysis which faces those who believe in the "sensation". It has merely transformed it into another set of coördinates.

In conclusion, it may be of interest to note that Watson, who

[6] See Dontchef Dezeuze. Op. Cit.
[7] *Psychology*, p. 307.

in the knowledge of the writer was the first to propose the conditioned reflex as the unit of habit, seems now to distinguish between them. He speaks of a conditioned reflex level occurring before the habit level in a child's life.[8] This seems to point to something like the thesis of this paper, that the conditioned reflex is a simplified type of habit rather than a unit. Invaluable though Pavlov's mechanism is for the study of certain aspects of learning, it should then probably not be thought of as unitary, but rather as a limiting case to which, on simplification, habit approximates. If there is such a thing as a unit of habit, this will probably be found in something more complex, possible along the lines of the natural, psychological articulations described by the animal experimentors.[9]

[8] *Psychology*, p. 291. This he later contrasts, as social habit, with true language habits. See p. 341.

[9] Corroboration for my main thesis is also to be found in the fact that many habits are not only relatively stable as compared with the Pavlovian mechanism, but are even extremely difficult to eradicate. Instances almost *ad infinitum* could be given not only from everyday life but from the pathological field. In fact a goodly proportion of the work of every psychopathologist may be said to be directed exactly towards such habit elimination.

THE COMPETITIVE CONSCIOUSNESS *

By IRVING C. WHITTEMORE

THE COLLEGE OF BUSINESS ADMINISTRATION OF BOSTON UNIVERSITY

1. *Introduction.* The technical interest in competition as a motive has hitherto obscured its significance as an introspective fact. A conscious attitude of rivalry toward one's fellows in the everyday life of business, of sports, and of social contacts is so commonplace as to have escaped the psychological notice of those best qualified to investigate it. In the course of experiments devised by the writer to probe the influence of a voluntarily adopted attitude of competition, as against mere group influence, on the quality and quantity of products turned out by subjects working on a semi-mechanical task,[1] the opportunity arose to make an intensive study of the "competitive consciousness". The method pursued and the facts revealed are here presented.

2. *The General Experimental Situation.* The observers, seventeen in number, of whom thirteen were Harvard men and four, Radcliffe women, ranged in age from eighteen to fifty-five years. The average age is 26.2 and the A.D., 5.0. They met twice weekly in groups of four and worked on a task involving the copying of passages from the daily newspapers with rubber letter-stamps impressed individually. In each normal experimental period of an hour, four five-minute tests of working ability were given. During some of these tests the observers competed, during others they disregarded one another's performances. The specific instructions for competition and noncompetition were as follows:

Noncompetition

> "Try to get as much work done as you can, remembering that both the quality and quantity of the work you do will count in your final score. Don't attempt to beat your fellow workers."

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* The experiments reported in this study were performed in the Harvard Psychological Laboratory. The report itself is condensed from a thesis entitled: "The Influence of Rivalry on Performance; an Experimental Study of Individual and Group Competition Under Approximated Industrial Conditions," on file in the Widener Library of Harvard University.

[1] A report of these experiments appeared in the JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY, October-December, 1924, pp. 236–253.

Competition

"Try to beat out your fellow workers, remembering that both quality and quantity count in your final score. You may use any method you see fit to employ in keeping track of the progress of your competitors. Compete!"

The introspective accounts of the competitive consciousness during the tests described were obtained in three ways. In some instances reports were requested at the conclusion of a test, the introspections to be based on that test alone. In others the subjects were warned beforehand that they were to be stopped during the test and asked to describe their experience. In still others no warning was given but they were required to report in the same manner.

3. *Preliminary Introspective Reports.* Early in the course of the experiments substantially the following questions were asked the observers immediately subsequent to a competitive test:

1. Were you conscious of competing?
2. If so, in what way were you aware of competition?
3. Was the competition directed against the other members of the group in general, against one of the group in particular, or against your own past record?

Nine observers replied to the first question in the affirmative with reservations in a number of cases. Thus:

Han:

"Yes, conscious of competing. Competition to increase both speed and neatness."

The negative replies, of which there were three, are typified by:

Jon:

"Distinctly was not conscious of competing. In fact I was interested in the procedure and was trying to figure out how to do it best. Was not trying to beat myself this time—but trying to get ready for next time."

In the majority of cases the answers to the second and third questions were not distinguished. Three subjects return replies suggesting an awareness of distinctive kinesthetic strains under the competitive effort, although no suggestion of these strains had been made to the observers at any time during the course of the experiments. Thus:

Mur:

"Just general wishing and *tension* kept up."

Gol:

"Competition by keeping tabs on Jon's efforts. Also by feeling of strain or effort."[2]

Of the nine observers who report an awareness of competition, four insist that the effort consisted principally in an attempt to beat their own previous records; two more are in doubt as to whether the competition is of this "auto" sort or not, one finding it tied up with a special effort directed against a single rival and the other confusing it with a general attempt to vanquish everyone; one insists on a particular rival; one competed "in general"; and one says that he was striving to find the best combination of speed and accuracy. His report seems to imply autocompetition.

Thus in seven out of the nine cases autocompetition, that is, an effort to surpass one's own best performance, plays a major or a minor part in the retrospective picture.

Samples of the reports implying autocompetition follow:

All:

". . . In this test I was more conscious of trying to produce a neater work than hers (*i.e., Was's*) and also than my own usual performance."

DeS:

". . . but the greatest drive was to beat my own record. I scarcely noticed others but certainly was conscious of the results."

Sample of reports implying other than autocompetition:

Was:

"Desire to exceed record of other members of class predominates efforts. Own record rather a matter of indifference."

The reports noted thus far were all made early in the experimental series. None of the observers had as yet given up the hope that he might eventually hit upon some way of combining speed with accuracy which would enable him in the long run to meet his competitors with some chance of winning over them. This, then, was a period of adjustment. Later reports will show that what the observers are describing as a consciousness of competition is not so much a particular sort of consciousness, not a

[2] If the phrasing of these reports seems naïve, it must be remembered that the observers were not attempting a thoroughgoing descriptive report in the sense in which Titchener, for example, uses that term (See Titchener, E. B.: Description vs. Statement of Meaning, *Am. J. Psych.*, XXIII, 2, 165–182), but were merely giving the writer some cues as to the conscious meaning of competition in everyday language.

sensational or perceptual process at all, but something corresponding to a determining tendency, the awareness of which is almost always *ex post facto*. Occasionally it may break out into immediate experience, as is suggested by rare mention of feelings of tension and the like, now and then an affective or emotional tone which colors the consciousness of the acts involved in performing the task set, but it will become evident when further introspections of a different sort are examined, that these immediate experiences of a perceptual or emotional sort are not the fundamentally characteristic phases of the conscious experience called forth by the competitive effort.

The tendency, then, is what the observers are describing in the reports of experience given above. And this tendency seems to involve two factors, an autocompetitive attitude on the one hand, and a sociocompetitive one on the other. Even the reports which insist on autocompetition as the principal thing of which the observer was aware imply that the reason for the autocompetition lay in the desire to win out in the long run. Lan, who returns a laconical "No" to the question: "Did you experience a consciousness of competition in some form or other?" and Cut, whose reply at first glance would seem to indicate intractability, nevertheless return later descriptions which clearly indicate that consciousness of competition was not entirely impossible to them. Cut almost in the next breath, after denying an awareness of competition in any form says:

"Tried to see why my work was so sloppy."

Surely here is a cue to the fact that she, too, was seeking an adjustment which might enable her to compete on better terms later on. It seems to imply *some* sort of competitive effort.

The subjects, then, were determined by the instructions to beat their fellows. They experienced considerable difficulty, however, in the attempt to keep track of the progress of rivals, hence the competition took the form of an effort to work as rapidly or as well as possible and to wait until the close of the test to see how rivals had progressed. The setting is sociocompetitive but autocompetition governs the productive acts of the moment. Neither sort of competition is continuously present in consciousness in sensational terms, but sensational or emotional processes sometimes rise to attentive clearness.

Occasionally a subject attained some notion of the progress of his competitors by glancing quickly at their work or by comparing

the rhythm of his own performance (usually auditorially) with that of the others. Thus:

Jon:

". . . Tried to beat Gol. Glanced over every now and then to see how far he had got. Noticed when he stamped his ink-pad."

Hoff:

". . . Occasionally I found rhythm coinciding (*with that of others*) and tried to beat the rhythm."

All:

"Conscious of competition by peripheral vision of arms swinging, noise of stamp, and by furtive glances at the papers of co-subjects."

A second matter of interest in connection with these early reports is the question of the possible decay of the competitive attitude. The observers were asked to report whether the "consciousness of competition" seemed to "die out". Of the eleven observers who answered this question three only report that the awareness of competition dies out during the test, and in all three cases with some qualification. Thus:

Was:

"Not conscious of competition after having got a few letters ahead. Was then only conscious of desire to improve method—maintain speed." (N.B.—*Autocompetition remained.*)

Bee:

"Yes. (*i.e., Competition died down.*) Tired."

The others indicate that they felt the competition to be just as strenuous at the close of the trial as when it began, and in some cases there is even evidence that the awareness of competition seemed to grow more vivid as time progressed. Thus:

DeS:

"The stimulus to competition lasted throughout. Also speed enhanced by autocompetition."

Hoff:

"It lasted. I got sore on upsetting letters in the box, and the competition died only for a few moments." (N.B.—*Isn't the "soreness" an indication that competition didn't "die"?*)

The usual opinion seems to be that, except under extraordinary circumstances, the competitive attitude either remains the same or slightly increases in strength.

4. *The Subject-Matter of Consciousness in Competition and Noncompetition Contrasted.* The next step in the investigation involved stopping the subjects in the middle of some test and asking them to report what conscious experiences they were having, in the attempt to discover whether there is a recurrence of some characteristic process which may be considered the mark of competition. The writer wished to discover whether the train of thought which a subject pursued during the course of the competitive tests was usually concerned with the act of printing, whether it more often amounted to some comparison which would give a clue to an awareness of competition present at the moment, or whether it usually was irrelevant. The reports to be discussed were called for immediately after the close of competitive tests. In some of these tests the observers worked in pairs.

Of a total of 65 "ideas" present in consciousness as reported by the various observers, 15 were irrelevant both to the task of printing and to competition. Examples of these irrelevant trains of thought are given below:

Bee:

"Thinking of trip to France in February—perhaps."

How:

". . . that I'd get to lunch when it was jammed."

Mur:

". . . of Bee's eyes." (!)

Thirty-four reports indicated that the observers were at the time concerned with some phase of the work at hand. From this group have been excluded all introspections which indicate a consciousness of sociocompetition. Any element in the reports which implied autocompetition was sufficient to bring them under this heading. Thus:

Hoff:

"Scrapping for speed, helter-skelter."

Gol:

"Wondering if it would be possible to make up lost time."

"Much amused at my error in spelling combined with flight of Miss Jon's letter-block."

Jon:

"Had just made a mistake—was saying 'Oh, dear' and thinking what a mess I was making."

Fifteen reports imply awareness of the efforts of others, consequently sociocompetition. Thus:

Gol:

"Wondering if it would be possible to make up lost time and catch up with Miss Jon."

Han:

". . . thought that I was keeping up well with Gol."

Bee:

"This pounding is more efficient—hand bounces each—goes down harder—why didn't I adopt it earlier? Afraid of breaking blocks? No, that's rationalization—yet—I'll go faster this way—almost imitation—social stimulation by violent movement."[8]

Further evidences appear in the reports just cited that the observers often have actual cues to the progress of competitors, that auditory rhythms seem to be peculiarly efficient stimuli to sociocompetitive effort, since there need be no distraction of the visual attention from the work at hand in order to perceive what is going on.

Reports were also called for immediately after the close of certain *noncompetitive* tests in the attempt to discover whether there were marked differences in the frequency of occurrence of the different classes of "ideas" which the writer has called (1) irrelevant, (2) autocompetitive, and (3) sociocompetitive. Irrelevant "ideas" are discovered with greater relative frequency in the noncompetitive tests. There were ten of these to seven whose content had something to do with progress in the work at hand. Interestingly enough Hoff and Han report actual awarenesses of competitive effort in noncompetitive tests. All introspects after an "NC" test as follows:

". . . I was aroused by the idea that other members of the group were secretly competing. I feel this particularly in the case of Hoff. It is a competition 'forced' on me because others, particularly he, surreptitiously compete."

In later reports the observers were therefore specifically asked whether in noncompetitive tests they ever experienced "feelings" of competition. These reports are to be considered presently.

The reports of irrelevant "ideas" in noncompetitive tests are in nowise different from the examples listed above under com-

[8] An apparent introspective awareness of what Triplett calls the "Dynamogenic" factor. See Triplett, N.: The Dynamogenic Factors in Pace-Making and Competition, *Am. J. Psych.*, 1897, IX, 507ff.

petition and no further samples need be given here. The same remark applies to the reports implying autocompetitive effort or awarenesses of attention to the work itself. Hoff's introspection implying competition in a noncompetitive test is as follows:

"Having rather soiled appearances, I went after speed."

Han's is given below:

"I was thinking of the relative speed of myself and Jon and thinking I was printing the same letters as she was."

From the reports summarized above it appears that in noncompetitive tests there is a tendency for the subjects to divorce their consciousness from the task at hand. Irrelevant "ideas" replace the awarenesses of the progress of the work at hand and of sociocompetitive effort which are characteristic of the consciousness during competitive tests. But there is some indication that there exists a tendency to continue competition into the noncompetitive trials.

5. *The Possibility of Competition in Noncompetitive Tests.* In order to discover roughly what proportion of the observers in any one test were likely to compete during a period the instructions for work in which stressed noncompetition, the writer explicitly asked the observers whether they felt any tendency to compete during certain noncompetitive tests. No warning had been given that such a question might be asked.

Three report either auto- or sociocompetition during such tests; two insist that there is no tendency to compete; two more indicate that the attitude during noncompetitive tests is different from that during competitive tests, that there is a change of set both toward speed and quality, but seem to imply that the effort to get ahead fast and well still persists, though it probably does not involve any comparisons with present or past performances, hence can not be said to amount to true competition either of the auto- or socio-kind. In all other cases there is doubt. The observers are unable to report on the matter at all.

The bulk of the evidence indicates that competition of one sort or another often enters into the attitude adopted during noncompetitive tests. This fact suggested to the writer that it may be possible that the productivity of the subjects is greater in noncompetitive tests which follow directly after tests performed under a competitive set than elsewhere. Here the chances that some feature of the competitive attitude may be carried over into the noncompetitive test are greatest. The problem is to discover whether the average quantity of product turned out in noncom-

petitive tests which immediately follow competitive tests is greater, less than, or approximately equal to the average quantity of product turned out in noncompetitive tests which do *not* immediately follow competitive tests, *i.e.*, which either come first in an experimental period or which follow other noncompetitive tests.

On the basis of 21 NC–following–C tests, and 59 NC–not–following–C tests the following values are obtained:

Average quantity, NC–following–C.....	77.2
Average quantity, NC–not–following–C.....	79.4

Thus it appears that there is no tendency for elements of the competitive attitude to carry over into immediately subsequent noncompetitive tests in such manner as to affect the speed of performance in such tests, in spite of subjective reports indicating an awareness of competition. In fact a very slight advantage in quantity rests with the noncompetitive tests that come either first in a period or else follow other noncompetitive tests. The average percentile increase in competitive tests on a task of this sort is 26.[4] Here the advantage for the noncompetitive tests which do not follow competitive tests is about 2.8 per cent.

Since certain tests which come first in an experimental series enter into the value for NC–not–following–C tests as given above, the question arises: does the freshness of the observers during these tests account for the slight superiority in speed which appears in the value mentioned?

On the basis of 59 first noncompetitive tests and 29 later noncompetitive tests the following values are obtained:

Average quantity, first NC tests.....	74.4
Average quantity, later NC tests.....	76.4

There is no evidence, then, that the excess in quantity of NC–not–following–C tests mentioned above is due to the inclusion in the calculations of data secured in noncompetitive tests which came first in an experimental period, for such data tends to pull down the average rather than to raise it. The excess corresponds to something like relief from the strain of competition rather than to freedom from fatigue.

Although, then, as the subjective reports indicate, there may sometimes be a carrying over of the elements of the competitive attitude into noncompetitive tests, this transfer is not sufficient

[4] Reference is to the writer's previous article in this Journal: The Influence of Competition on Performance; J. Abn. and Soc. Psych., 1924, XIX, 3, 236–253.

in extent nor frequent enough in occurrence to occasion a rise in productivity even in those noncompetitive tests which immediately follow competitive tests.

6. *The General Questionnaire.* At the close of the entire series of experiments a questionnaire was submitted to all the observers, who had accumulated by this time much experience in attitudes both of competition and of noncompetition. The inquiries on the questionnaire to be discussed here are limited to those which developed points either new or supplementary to the facts about the consciousness of competition already brought forward.

The first question was as follows:

> "State as carefully as possible what you think your attitude has been in approaching this experiment. Has it been a pleasant social gathering, a bore, a game, or what?"

The purpose of this question was to ascertain to what extent either social contracts or the element of competition tend to relieve the monotony of a simple task oft repeated. Of the twelve subjects reporting, one alone admits that the experiment was at times merely a bore, but he qualifies this statement by saying that his disgust at the monotony came at about Christmas time when he was not well. Two others feel that the work is tiresome, but that the pleasantness of the group associations during experimental periods makes it endurable. Seven observers think that the game element is the characteristic thing about the experiment as a whole. Nine consider the experimental periods to be pleasant social gatherings. Four speak of the experiment as "routine" or a "job that has to be done", thereby implying that the affective response called forth by the experimental situation is in them rather indifferent. In so far as any general conclusions regarding the social or competitive effects on the pleasantness of an otherwise monotonous task may be drawn, one may say that group work is less boring than that done alone, and that the competitive element, conscious or unconscious, is in part responsible for the relative freedom from monotony. Typical answers to the first question are given below:

Hou:

> "It has been a pleasant gathering—because of the company, but tiresome work."

Mur:

> "A very tiring game, made endurable by the company."

The second question dealt with the attitudes assumed by the subjects when they first began to compete. It was worded as follows:

> "How did you begin competing? By competing with any one in the group, the group as a whole, yourself, etc.?"

The answers to it and to Question 10:

> "Do you think that any 'competing' which you may have done was a matter of working at your maximum capacity, regardless of your neighbors; a matter of autocompetition; a matter of competition with an individual; or a matter of competition in general? Explain in detail."

will be considered together.

It becomes apparent from the answers to these questions that there are two ways in which the subjects adjust themselves to competition. Six report that they began by trying to do the best they could to increase their own scores, by seeking to improve their method up to a point where they find there is a chance of competing on fairly equal terms with other members of their several groups. The competition is at first autocompetition.

Four compete tentatively with the group at large. So far as can be discovered from an examination of their introspections their attitude differs from that of the observers mentioned in the preceding paragraph in that they are striving to get ahead "on their own" but are most of the time dimly aware of the fact that others are trying to beat them out. Just so soon as they find what subjects have a productive capacity about equal to their own they shift their attitude to one definitely concerned with vanquishing some particular rival. None of the subjects, with one possible exception, continue after the first tests to compete "in general".

The ambiguity between autocompetition and competition with the group at large is most strikingly illustrated in the reports on the questions under consideration. In general one may say that competition starts out in a tentative fashion, being either auto- or socio-, and that it shifts later to competition with some particular opponent. This opponent need not always be the same for any one observer. There is considerable evidence that the observers shift their attention from one to another of their opponents, according to the progress of those opponents. It is usually the rival whose record is most nearly like that of the observer himself against whom the competitive efforts are directed. This discovery is particularly interesting in connection

with Moede's observation that the more equally matched competitors are the greater will be the influence of rivalry upon their accomplishments, and that as soon as two contestants know the disparity between them the weaker gives up trying.[5]

Samples of the reports on which the above remarks are based follow:

All:

"Competition has usually been directed towards some *one* in the group. Finding myself always outclassed by Was, I have turned to DeS or Hoff. Failing to exceed one subject in score, I usually thought, 'Well, anyway, I beat so-and-so.'"

Kar:

"I began to compete against the group. After the first few results I noticed my position in order of ability and then competed against the man just above me."

Although the social situation in the group is a stimulus to competitive effort, this effort amounts principally to autocompetition during the course of the test itself, especially in the later portions of the experimental series. Reference to individuals or to the group as a whole comes in the post-test comparisons of success. Autocompetition enters into the situation at some time or other for every one of the twelve observers. Five are occasionally aware during the test itself of the progress of competitors. Three mention the ultimate reference to the group as a whole. Eight are more interested in the success of one rival than in the achievements of every member of the group relative to one another.

Examples of the reports emphasizing autocompetition in the competitive attitude are given below:

DeS:

"A large part of competing was done regardless of proximity of neighbors. The general consciousness of public announcement of results was the most potent urge. Looking at other people while working entered into my competing very rarely. Would say mainly an individual attitude of improving as well as a desire to maintain or improve standing on daily announced results."[6]

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[5] Moede, W.: *Der Wetteifer, Seine Struktur und Sein Ausmass; Zeit. f. pad. Psych.*, 1914, XV, 353–368.

[6] The results were actually announced after each test.

Kar:

> "Think that competing was matter of working at maximum capacity regardless of others." But the knowledge of others' success "was added stimulus—even though I always thought that I was working at maximum capacity regardless of them."

It may be said, then, that autocompetition is the most important element in the competitive setting on such tasks as the one employed in these experiments. The competition is only social as a matter of reference. Sometimes the awareness of one's opponents breaks through into the consciousness of the general situation but the reference to the progress of rivals is more or less incidental. In any one test period the tendency for the break-through to appear is more likely to be determined by a single opponent than by the general group situation. This phenomenon is doubtless due to the fact that the social reference when it comes usually concerns that competitor whose skill most nearly approaches one's own.

To the fourth question:

> "Which type of work did you find the pleasanter, that done under noncompetitive or under competitive conditions?"

five observers reply that they prefer the competitive work, five prefer the noncompetitive, one prefers sometimes one and sometimes the other, and the twelfth is indifferent. There is no clear preference for the one type of work over the other.[1]

Question 7 aimed to determine whether awarenesses of competition lapsed in vividness and in frequency as time wore on. Its wording was as follows:

> "Did you feel that the impulse to compete wore off or increased in intensity as time went on? Did it do either?"

A priori one might expect that once the novelty of competitive striving on the experimental task is supplanted by a habitual adjustment to the setting for competition the awareness of effort would begin to subside. Precisely this occurs according to the reports of five of the observers. Two of them mention the fact, however, that due to some adventitious circumstance the aware-

[1] Meumann in a study of school children found that 80 per cent preferred class work, where rivalry was possible, to work alone; but he did not compare noncompetitive class work with competitive class work. See Meumann, E.: Haus und Schularbeit; Leipzig, Klinkhardt, 1914.

ness of effort may reappear again vividly, that the level of effort in terms of conscious acknowledgment of that effort runs in the form of a wavy curve. Thus:

DeS:

"It wore off and comes back from time to time . . ."

Was:

"Found impulse to compete less intense as time went on—but found it harder to shake off when noncompetitive conditions were declared" (*i.e.*, shrinkage of the contrast between C and NC noted).

Five observers report an increase in the "spirit of competition". All but one belong to that group which entered the experiment immediately subsequent to the midyear examination period. Only Hou of the older group experiences an increased awareness of the competitive nature of his efforts in rivalry. He explains this increase in terms of a better acsuaintance with his coworkers. He felt himself more a part of the group with which he met and this feeling of closer social contact determined him to respond more vigorously to the group situation.

The other four explain the increase in competitive spirit as due to an approach on their part to the records of the more experienced members of the group. They were still in that portion of the adjustment period which held out some hope to them of eventually vanquishing everybody. Thus:

Kar:

"Impulse to compete kept increasing with me because I felt that I was improving and as yet had not got out of the experimental stage—felt I could improve much more."

It is fair to say that in the long run the competitive spirit droops. During the adjustment period, when the hope still remains in most subjects that they may rise to fame as competitors, the impulse tends to increase. Once the practice curve has straightened out and each knows about where he stands relative to the rest of the group there is no longer the stimulus to competition which once existed.

7. *Systolic Blood Pressure in Competition.* There had been occasional suggestions in the introspections of the subjects that an emotional element entered into the "competitive consciousness". Thus:

Gol:

"Emotional consciousness of opponents greater when working alone than when working with this same opponent as a partner."[8]

Jon:

"Wasn't at all calm, my hand trembled with excitement."

It occurred to the writer that readings of systolic blood pressure taken on the subjects during periods both of noncompetition and of competition might be expected to yield a clue to the psychophysiological nature of rivalry. This method of expression was selected in preference to others because several investigations going on simultaneously in the Harvard Laboratory were employing it and the opportunity for developing a satisfactory technique seemed exceptionally good.

A modification of the experimental conditions became necessary in order that blood-pressure readings might be taken. All the subjects were required to rest the left arm in the lap. The experimenter sat just to the left of the one subject on whom he worked. To the bared left arm of the latter a Sanborn spring sphygmomanometer was attached over the brachial artery. The return of the pulse to the radial artery was determined tactually. Readings were taken before and after tests to establish a norm, and on the average four times in each five-minute period of competition and noncompetition. In their replies to a questionnaire the observers reported that the wearing of the sphygmomanometer did not seriously distract them from the work of printing.

The discovery by Marston[9] that concentrated study for periods of forty to fifty minutes fails to bring about a rise in blood pressure is particularly significant in connection with the experiments on competition. For the task employed here involved, as has been described, the reading and copying of passages from the daily press. Marston concludes that intellectual work of a far more strenuous sort than this is accompanied by a rise in blood pressure of never more than eight milligrams of mercury, and rarely more than six milligrams. There is no danger, then, that the "mental" aspects of the task employed in these experiments will obscure a rise in blood pressure due to any emotional element in the attitude of competition.

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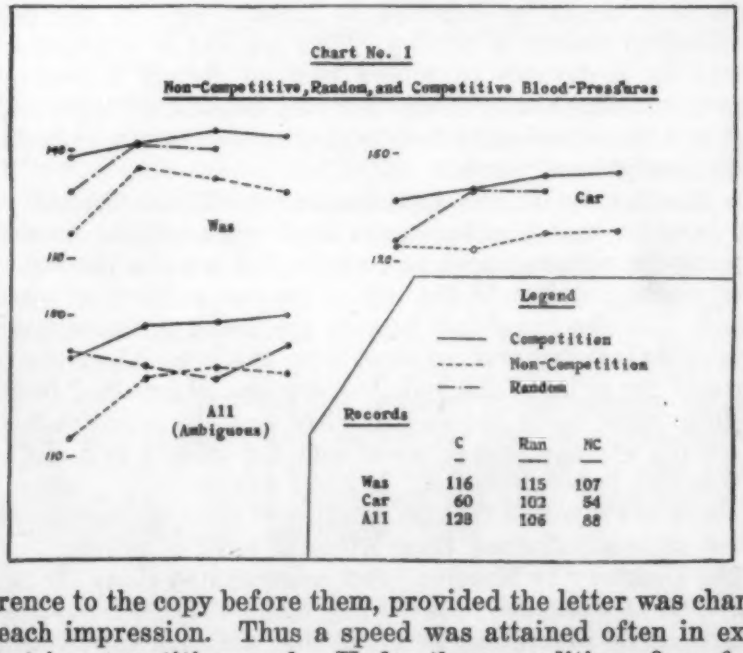
[8] Referring to certain tests in which the subjects worked jointly in pairs.

[9] Marston, W. M.: Systolic Blood-Pressure Symptoms of Deception; *J. Exp. Psych.*, 1917, II, 2, 117–163.

There is no question of a heightened blood pressure in the competitive setting. Every one of the fourteen subjects shows it. But whether the rise is due to greater muscular effort during periods of competition, when productivity is high, or to some emotional tension not involved in the action itself, could be determined only when some check on the degree of muscular activity is provided. A series of control tests was instituted in which the subjects were allowed to print any letter in the box without



reference to the copy before them, provided the letter was changed for each impression. Thus a speed was attained often in excess of that in competitive work. Under these conditions five of nine subjects on whom the experiment was tried have a blood-pressure curve higher in competition than when they are selecting the letters at random, although actually an equal number or even more individual letters were printed under the latter condition. Four cases are ambiguous. Typical curves are presented in Chart No. 1.

The suggestion is strong that circulatory changes characteristic of the emotions occur in competition, but the evidence is, of course, meager. It is unfortunately impossible to continue the experiments at the present time.

8. *Summary of Conclusions.* 1. Competition on a task commences with an adjustment period in which the subjects either

report (a) a conscious effort to orient themselves to the task, to build up both speed and quality in preparation for future competition of a social sort, or (b) a conscious attempt to discover their probable position in ability relative to opponents, in preparation for more intimate rivalry.

2. Competition with the group at large is less frequent than competition with a particular individual. That member of the group whose skill most nearly approaches the skill of a given subject is the one who tends to be singled out as his principal rival.

3. Autocompetition plays a large part in the competitive efforts of all subjects; it plays the principal part with some of them.

4. The consciousness of competitive effort is *ex post facto;* rather in the nature of a recognized attitude than an immediate awareness. It resembles the acknowledgment of a determining tendency. Occasionally it breaks through into consciousness as an immediate experience of the efforts of others or as a recognized comparison, at the moment, with the subject's previous performances.

5. In most cases the competitive spirit rises during the period of adjustment. It dies out in the long run.

6. Elements of the competitive attitude sometimes carry over into noncompetitive periods of work, but there are no objective indications of a rise in productivity corresponding to this persistence.

7. The subject-matter of consciousness during competition includes irrelevant ideas, awareness of autocompetitive effort, and sociocompetitive references. The proportion of irrelevant ideas is higher in competition than in noncompetition.

8. A social situation relieves the boredom of a simple task oft repeated. In competition the "game" element is responsible for some of this relief.

9. About as many subjects indicate a preference for noncompetitive as for competitive work. (There is no relation between preference and success.)

10. There is some evidence that most subjects undergo physiological changes leading to a rise in blood pressure, probably attributable to an emotional element of excitement, during periods of competition.

AUTOMATIC WRITING COMBINED WITH "CRYSTAL GAZING"

By MORTON PRINCE.

IN a recent number of this JOURNAL[1] Dr. Anita M. Mühl records some interesting experiments in recalling forgotten incidents and inducing subconsciously written fantasies by the method of combining crystal gazing synchronously with "automatic" writing. This method I devised for the study of the mechanism of hallucinations,[2] and Dr. Mühl follows this method, excepting that certain important details of my technique she apparently overlooks. These, details, which I shall presently mention, are important for the interpretation of the image-producing process and the relation of the images to the "involuntarily" written script. It is for this reason that I am led to comment upon Dr. Mühl's experiments and conclusions which, as she points out, are impossible to reconcile with mine. My motive is not to criticize but only to explain the reasons for the differences in our findings and conclusions and to this extent show how these differences have arisen. I also want to point out certain fundamental principles involved in such experiments so that those who may wish to repeat the experiments may be guided in their researches. Unless these principles are recognized all such experiments are bound to be fallacious.

The method consists in inducing so-called crystal pictures in a subject and at the same time, while the subject is seeing and describing orally the images as seen in the crystal, have the hand produce so-called "automatic", that is, subconsciously written, script. With a proper subject the personal consciousness is not aware of what the hand is writing. The problem is, what is the relation, if any, between the script and the images? Of course the use of a crystal is not essential; that is only a matter of technique and aids as a suggestion that images will appear and in concentrating the attention. Some subjects need only look into space or at a wall or any object. In my experiments I enveloped the head

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[1] Vol. XIX, No. 3, Oct.–Dec., 1924.

[2] An Experimental Study of the Mechanism of Hallucination. British Journal of Psychology, Medical Section, April, 1922.

of the subject with an opaque cloth. The description of the images given orally is taken down verbatim to dictation. The pictures are in principle, of course, artificially created hallucinations.

Now, an important detail of the technique, as I will presently further point out, is that the words of the script that are written during the emergence of the images shall be accurately time-checked. For this purpose the subject should precisely mention the exact moment when the image appears and the moment when it disappears. These time points are then marked on the script while it is being written. In this way it is possible to correlate in time the words of the script with the image, and also in content, if there be any such correlation.

After these two records are obtained the description of the image and the synchronously written words of the script are compared. For this purpose the two records may be conveniently set off against one another in parallel columns—the images in one and opposite them the script in the other. We can then easily see whether the images are such as we would normally expect to be in the content of the synchronously written "ideas" which are set off by the time checks.

This precise technique should be followed because in order to correlate in time-relationship the images, or hallucinations, with the script it is obviously necessary to check the exact points in the script when an image emerged and when it disappeared. When this is done we can determine, as I did in my experiments, what logical relationship, or relationship of content, if any, there may be between the images and the written "ideas" of the script and perhaps identify the two in content. In this way, also, it is possible to draw conclusions as to whether images and script are the product of one single process or whether they represent two different processes. Thus the content of the hallucination and of the script will sometimes enable us to determine that there is no logical relationship and that there may be two differentiated processes.

For example, let us suppose the hallucination is of a kitten playing with a ball while the script describes a ship on the ocean. Here plainly we would have to do with two different processes. But suppose that with this hallucination the script described some other behavior of the kitten, the process determining each may or may not be the same.

Suppose, again, the images (hallucinations) are of a small

child sitting on the floor alone in a room playing with a white kitten; the kitten has a fit and runs away; the child is frightened, appears to scream and call for some one. And suppose that synchronously the hand subconsciously writes, "I remember that when about five years of age I was sitting on the floor alone by myself playing with a white kitten; the kitten had a fit and ran away; I was frightened and called 'Mama!' but nobody came." Here we have very precise correlation in time and content. And obviously the images were just such images as theoretically would normally be incorporated in the memory (thought) of the childhood incident. The inference, therefore, is justifiable that the subconscious process producing the images and that producing the script are one and the same. If, further, by introspective methods the process producing each could be recalled to awareness (*i.e.*, to the personal consciousness) and this introspective testimony corroborated the inference and further could describe how the images emerged into awareness as the hallucination, and even the exact moment that this occurred, then we would be justified in accepting the inference as sound. But obviously a very precise and accurate description of the images and a very precise correlation of them in time with the words of the script is necessary in order to determine the relationship of the processes producing them.

The subject also should be a good describer; that is, should be able to describe precisely and accurately, without extraneous words, the images as they appear, just as any accurate observer would describe any picture or scene he is looking at, without "reading into it" his own interpretations, and avoid general statements as much as possible.

Dr. Mühl found as to the "time relationship between the written record and the crystal picture" that "the only consistent thing about this relationship proved to be the fact that it was wholly inconsistent". I am not surprised at this. Although mention is made that the words of the written records were "checked" in relation to the images as described by the subject, it is not stated how this was done, nor are such words checked in the published record. I may be misled by the form in which the experiments are reported, but as the record appears there is no way of judging the time or content relationship.

Accurately *controlled* experiments in automatic writing are not as simple as they may seem. Consequently various errors enter and vitiate the results. We are thus unwittingly misled in

our conclusions. One important source of error, as every one knows who has had extensive experience (and I have records of hundreds of observations), is that subconsciously written script may be produced by several different subconscious processes in the same subject without there necessarily being any relationship between them beyond that of an underlying motive. It is generally assumed that automatic script is written by a single entity—the subconscious mind. There is no "subconscious mind" or "self" in the sense in which these terms are popularly used even by psychological writers.[8] At least there are no facts which justify such concepts. There are subconscious processes any of which may take on autonomous activity and determine "automatic" and other kinds of behavior (including conscious mental processes of the personal consciousness); and among these activities may be automatic writing and hallucinations. Theoretically, therefore, such script may be written by any number of different, and more or less independent, systems of integrated dispositions; *i.e.*, subconscious processes. Practically it is not uncommon to obtain script produced by two, three, four, or more such processes in the same individual.

In consequence of this it is not uncommon to find, when using subconscious writing to explore the mind, that the script will record contradictory and irreconcilable statements. But in such cases it will be found that the writing process has, without other indication of the fact, suddenly shifted to another system of dispositions, perhaps with different memories, different motives, and different acquired knowledge. Until this shifting is recognized the content of the script becomes extremely puzzling. Even two or more different systems of dispositions may be in activity at the same moment, one writing with one hand and another with the other, or doing something else (*e.g.*, seizing the pencil used by the other hand, or otherwise impeding the writing); or one may be writing and from the other images may arise into consciousness as an hallucination. This is in accord with the fact that during automatic writing or a crystal vision, when produced singly, two different processes are always and necessarily in synchronous activity; one that of the personal consciousness (the alert consciousness) and the other that which produces the script or vision, as the case may be. Hence the latter may be a co-con-

[8] Excepting, of course, in some cases of such extreme dissociation as result in "double personalities" where one is coconscious.

sciousness. There is nothing extraordinary about multiplying the processes in synchronous activity. All this must be borne in mind when attempting to correlate the script with the hallucinatory images in such experiments as we are now considering. It, therefore, well may be that no correlation whatsoever may be found. On the other hand it is possible, when such is the case, to find the other particular process which is responsible for, or may be correlated with the images, although this may not be the first script produced. This, for example, was the case in observations VII and VIII in my study of the mechanism of hallucinations above alluded to.

It does not appear that Dr. Mühl took these important points into consideration. Thus, as I interpret the first example which she gives of her experiments, there is no real correlation at all between the script and the "crystal picture"; that is to say, between the *process* which produced the script and the *process* which produced the picture. This experiment is cited as an example of a recall by automatic writing of a memory of an episode with a kitten which had occurred about a month before. But as I interpret the writing, it is not a memory at all but a description by subconscious processes of what the subject, that is the personal consciousness, is seeing in the crystal. The language of the script indicates this: "and now I *see* a kitten playing with a spool, and it is pushing with one paw and then with the other, and then running after it hard", etc. This is the language of the script which is obviously describing what the subject is seeing, for the oral description of the images runs, "I see a kitten: It is a cute little fuzzy one and is jumping around and playing with something", etc. (Here, by the way, the oral description by the personal consciousness is not as precise as the written subconscious description. It is rather a comment: "a cute little fuzzy one".)

As every one knows, it is common, when experimenting with automatic writing, for the subconscious process to describe what the subject is thinking or seeing or hearing or feeling at the moment (the personal consciousness, of course, does not know what the hand is writing). Such subconscious descriptions I have obtained over and over again. If the script in the example given by Dr. Mühl had been a true memory the script would have described, according to the records of my experiences, the episode as any one describes a past experience and would not have written "I see", etc. It would have written something like, "About a

month ago I was playing with a kitten. The kitten began to play with a spool", etc. The theme would be the same as that of the emerging hallucinatory images, but, as I found to be the case, the image would be correlated only with the "perceptions" as expressed by particular words of the scripts. In the example cited by Dr. Mühl, there were, as I interpret the phenomena, two subconscious processes, not one—one irrelevant doing the writing and one undiscovered which produced the images—and the attempt at correlation was with the irrelevant process. It is not surprising that there was a discrepancy between the findings in this and other examples, through which the same fallacy of technique runs, and those of my experiments. In some examples, however, the script is obviously a true descriptive memory and might be correlated with the hallucination if the synchronously written words had been checked in the report.

Accordingly it must be borne in mind that the oral, volitional description of the images, *being given by the personal consciousness,* is by a separate and different process from the subconscious "automatic" one which produces the images as well as from that which produces the script. That is why the oral description often rambles, comments and philosophizes and must be restrained to a precise description of the images. Otherwise correlation is difficult or impossible. What we are trying to correlate with the subconscious script process is not the personal consciousness but the subconscious image process.

Another source of error is the naïve acceptance of script memories as true memories and not hallucinatory memories, as they often are. That is to say the apparent memories are merely expressions of beliefs, not of actual experiences. This is very commonly true of our conscious memories. We often remember as our experiences what really has been only told to us, or what we have learned in other ways. The same is true of artificially induced images ("crystal") and spontaneous hallucinations. Thus, I have frequently induced images of apparently actual past experiences but which were shown to be hallucinatory memories and only the expressions of belief.

Obviously, to me at least, Dr. Mühl's record of a memory of an episode when the subject was only fifteen months old is of this hallucinatory character. Aside from the *a priori* unlikelihood of a person having a memory of this early period of life, the content of the memory on its face is preposterous. The memory requires

that a fifteen-months-old baby recognizes "fur" and a "lion" as such—certainly a very experienced baby!

In view of all these sources of error, as I said at the beginning, controlled experiments in automatic writing and induced hallucinations and their interpretation, are not so simple as they may seem.

I come now to the conclusions which Dr. Mühl draws from her experiments. We find among them the following:

5. For the average adult, images have merely the minor function of aiding the abstract mental processes; and it is only when the thought processes come to a standstill, or are reduced or blocked, that the train of thought seems to go back to the track of visual images.

6. The above sounds somewhat regressive. According to Jaensch, as quoted by Storch, the use of percept images by adults represents a primitive level of intellectual development.

7. This interpretation seems somewhat difficult to reconcile with Prince's idea, that these experiences, so far from being a regression of thought, are really elements in highly developed adult thought processes. And this brings me to my conclusion that

8. . . . Obviously then, percept images indicate on the one hand neither an entirely regressive state; nor on the other hand a totally progressive condition. They may be, with intelligent direction, useful and constructive; or, due to failure of direction, harmful and destructive.

In view of all that has been said above in regard to technique and the general principles governing subconscious writing, it is not surprising that Dr. Mühl has found it difficult to reconcile her interpretations with my own. It is impossible. This difference of interpretation in view of conclusions 5 and 6 would seem to be explainable in that the facts themselves elicited by our different methods are entirely at variance. But I find it difficult to understand from what facts Dr. Mühl's conclusion No. 5 is drawn. It seems strange, in view of all that is known of the behavior of dissociated mental processes, as exemplified in dissociated personalities, automatic writing, induced hallucinations (crystal visions), suggested post-hypnotic acts, etc., that it should be concluded that visual images develop only "when the thought processes [which?] come to a standstill, or are reduced, or blocked". I fear that such an interpretation is only an insistence that the fact shall fit the theory. And, above all, it is hardly

acceptable to make them fit the most questionable theory of Jaensch that the use of "images by adults represents a primitive level of intellectual development". The poetical and mystic mind will feel a blow in the solar-plexus when told that! It would seem, if I may be permitted to say so, that a theory already accepted has been read into the facts. We must take our facts as they are and if theory does not agree with them, so much the worse for theory and not for facts. Moreover, I cannot help thinking that, if I may speak frankly, Dr. Mühl misses the whole point of my theory and overlooks the facts of observation upon which it is based. The observations, of course, need to be confirmed and may be faulty, and in that case the alleged facts disclosed should be thrown into the discard, as I am ready to do, but until the observations have been shown to be erroneous, or new facts incompatible with the theory have been brought forward, the theory should stand as entertainable.

The theory which I proposed may be restated as follows:

1. (a) The subconscious process inducing the writing of a theme and that inducing the images may be correlated in that they are one and the same.

   (b) The images are the normal images pertaining to the subconscious "thought-processes" that produces the script (*e.g.*, if the thought-process is that of a kitten, the image is that of a kitten, as in everyday awareness. Whether such a subconscious process has the attribute of consciousness—a co-conscious thought—is a problem by itself and need not be here now discussed).

   (c) At a given moment, under certain conditions, the images pertaining to the subconscious thought-process emerge into the awareness of the personal consciousness and thereby become the hallucination (crystal vision). Or, stating the same fact and in different terms, under certain conditions the personal consciousness becomes aware of the subconscious images and thereby experiences the hallucination. *But* the remainder of the subconscious process continues outside of the field of awareness (*i.e.*, is subconscious) and continues to produce automatic script. (Sometimes, as every one ought to know, the whole subconscious process emerges into awareness and then the personal consciousness is aware of what is being written.)

2. Under such circumstances we find a correlation of time and content between images and script as would necessarily be the case, the apparent two processes being really only one.
3. That this is the true interpretation we find corroborated by the evidence furnished by the testimony of subconscious introspection, that is by the script, whatever this be worth. And I believe this testimony must be accepted as reliable until it has been controverted. Three different subjects gave entirely independent testimony to the same effect; each testified that there was, during the phenomena under investigation, a subconscious thought process in which images were elements; that at certain moments these images emerged into the awareness of the personal consciousness and became the hallucination. From consideration of this testimony of the first two subjects studied, on whom numerous observations were made, my theory was first evolved. It was to obtain further objective corroboration of this introspective testimony that the method of combining visual and auditory hallucinations with automatic script was devised and applied to the third case.
4. If the facts I have recorded be accepted as sound, then it is obvious that the images "are really elements in highly developed adult thought-processes", thoughts that produce the subconscious writing, and not a regression to a primitive level, because the automatic script manifests highly developed reasoning, imagination, feeling, etc., such, for example, as versification, novel-writing, mathematical calculations, argumentation, etc. Surely such intellectual processec are not primitive and are not a regression.

If the image-process is one that is entirely different from and independent of the script-process the regression theory might be entertained, but if they are the same process, then manifestly this is impossible in view of the highly intellectual level of the script-process. This is a very important consideration, but I can not help feeling that Dr. Mühl does not fully realize this fundamental principle involved in my experiments and theory and therefore the possibly true relationship between, or identification of, the two processes.

A GREAT ADVANCE OF THE FREUDIAN PSYCHOLOGY

By WILLIAM McDOUGALL

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IT is interesting to find that Professor Freud is less rigidly Freudian than many of his ardent disciples. Readers of Freudian literature will remember how in 1919 a group of Freudians ("Zur Psycho-analyse der Kriegsneurosen"—with introduction by Professor Freud), endeavored to show that the neuroses of the war might be interpreted in terms of the Freudian principles. In spite of much ingenuity the endeavor can hardly have seemed successful to any impartial reader. More recently Professor Freud himself, has frankly abandoned this attempt. In his "Beyond the Pleasure Principle", he acknowledges that further consideration of the war-cases has led him to profound modifications of his theory. The recurrent war-dream has very naturally been a difficult problem for those who would bring it within the range of Freudian principles. In order to do this, it was necessary to make it appear that the fear, which was so prominent a feature of the war-dream, was somehow a sexual derivative and that somehow, in some sense, the patient was feeling or attaining pleasure through his torturing battle-dreams. Freud has now frankly abandoned this endeavor, and has modified his theory in the following way.

The original Freudian theory was a strange mixture (it could not be called a blend) of three irreconcilable principles, each of which is the fundamental assumption of a distinctive type of psychology, namely, first, mechanistic determinism; second, psychological hedonism (implied in a thoroughgoing sense by "the pleasure-principle"); third, the hormic principle which sees the root of all activity in instinctive urges that are the primary prompters and sustainers of all thought and action.

The phrase "beyond the pleasure principle" is now used by Freud to denote the fact that he now recognizes the primacy of the hormic principle, that he now recognizes that instinctive urges work within us in relative independence of pleasure and pain. In this book Freud even comes near to recognizing that pleasure and pain are conditioned by the instinctive urgings, by their success and failure, and by their conflicts.

It is true that in an obscure way Freud has long recognized the inadequacy of the pleasure-principle and has embodied this recognition in a profoundly obscure term, namely "the reality-principle". This was held to be a sort of interloper whose agency could be invoked whenever the pleasure-principle was too obviously out of the picture. "We know that the pleasure-principle is adjusted to a primary mode of operation on the part of the psychic apparatus, and that for the preservation of the organism amid the difficulties of the external world it is *ab initio* useless and indeed extremely dangerous. Under the influence of the instinct of the ego for self-preservation, it is replaced by the 'reality-principle'; which without giving up the intention of ultimately attaining pleasure, yet demands and enforces the postponement of satisfaction, the renunciation of manifold possibilities of it, and the temporary endurance of 'pain' on the long and circuitous road to pleasure."

And in the following passage, he seems to recognize, in a partial and grudging manner, the truth that pleasure and pain are conditioned by and upon our hormic or instinctive strivings: "It is at the same time indubitable that the replacement of the pleasure-principle by the reality-principle can account only for a small part, and that not the most intense of painful experiences. Another and no less regular source of 'pain' proceeds from the conflicts and dissociation in the psychic apparatus during the development of the ego towards a more highly coördinated organization. . . . The two sources of 'pain' here indicated still do not nearly cover the majority of our painful experiences. . . . Most of the 'pain' we experience is of the perceptual order, perception either of the urge of unsatisfied instincts or of something in the external world which may be painful in itself or may arouse painful anticipations in the psychic apparatus and is recognized by it as 'danger'. The reaction to these claims of impulse and these threats of danger, a reaction in which the real activity of the psychic apparatus is manifested, may be guided correctly by the pleasure-principle or by the reality-principle which modifies this. It seems thus unnecessary to recognize a still more far-reaching limitation of the pleasure-principle, and nevertheless it is precisely the investigation of the psychic reaction to external danger that may supply new material and new questions in regard to the problem here treated."

Freud then goes on to recognize that the pleasure-principle is not fundamental, that there is in us something deeper than or

prior to the pleasure-principle, or, as I should put it, that pleasure and pain do but modify, in the way of promoting or checking, the fundamental urges of our instinctive nature. I do not say that Freud actually attains to this position; rather, recognizing that something is more fundamental than pleasure as a determinant of activity, he creates a new and primary instinct which he calls "the repetition-compulsion".

Contemplating the obstinate recurrence of war-dreams and allied repeated manifestations of strong primary urges, Freud proposes to recognize as the most fundamental tendency of our nature a "repetition-compulsion". "The new and remarkable fact, however, that we have now to describe is that the repetition-compulsion also revives experiences of the past that contain no potentiality of pleasure, and which could at no time have been satisfactory, even of impulses since repressed". Again, "It is a question naturally of the action of impulses that should lead to satisfaction, but the experience that instead of this they even then brought 'pain' has borne no result. The act is repeated in spite of everything; a powerful compulsion insists on it. . . . In the light of such observations as these, drawn from the behavior during transference and from the fate of human beings, we may venture to make the assumption that there really exists in psychic life a repetition-compulsion, which goes beyond the pleasure-principle. We shall now also feel disposed to relate to this compelling force the dreams of shock-patients and the play-impulse of children."

"This (the repetition-compulsion) seems to us more primitive, more elementary, more instinctive than the pleasure-principle which is displaced by it—to which we have heretofore ascribed the domination over the course of the processes of excitation in the psychic life."

After making this great step towards a truer psychology, Freud sets out upon a highly speculative inquiry into the nature and origin of the repetition-compulsion and finds the explanation of it in the astonishing conclusion that all instincts, except the sexual, tend or strive toward death. "They (the instinctive tendencies) thus present the delusive appearance of forces striving after change and progress, while they are merely endeavoring to reach an old goal by ways both old and new. This final goal of all organic striving can be stated too. It would be counter to the conservative nature of instinct if the goal of life were a state never hitherto reached. It must rather be an ancient starting point,

which the living being left long ago, and to which it harks back again by the circuitous path of development. If we may assume as an experience admitting of no exception that everything living dies from causes within itself, and returns to the inorganic, we can only say 'The goal of all life is death', and, casting back, 'the inanimate was there before the animate'."

This highly characteristic piece of reasoning, with its strange conclusion, is hardly one that compels assent. There is surely a very much simpler explanation of all the facts which Freud explains by means of his "repetition-compulsion." They all follow naturally from our fundamental assumption, if that assumption is the validity of the hormic principle. According to this principle every instinctive tendency is a tendency towards a goal of a particular kind, and however it be brought into play it tends towards that goal. Hence repetition is of the essential nature of instinctive activity. The repetitive character appears, it is true, most strikingly in those instances in which by repression and dissociation a complex has been formed, especially in the case of relatively simple complexes that are completely dissociated; for in such a complex some strong instinctive tendency, bound up with some dissociated memory, works without the guidance of the personality which would modify its manifestations upon each occasion. And so in these cases we get such striking repetition-phenomena as the fugue, the fit, the phobia, the terror-dream, the battle-dream, the tic, the obsession or the compulsive act, the recurrent nausea, etc.

What need then to follow Freud in his extravagant speculation leading to the conclusion that all our instincts (except the sexual) including the foodseeking instinct and the instinct of fear or self-preservation, strive toward death?

Freud asserts in the course of his discussion—"No knowledge would have been so important for the establishment of a sound psychology as some approximate understanding of the common nature and possible differences of the instincts." This is the thesis on which I have not ceased to insist since writing my "Social Psychology" in 1907, and I venture to think that if Professor Freud and his followers had taken it to heart from the first, we should have seen more rapid advance, with less of misdirected ingenuity proposing impossible explanations under the misguidance of a false dogma, the dogma of the all-dominance of the pleasure-principle, which is nothing other than the long discredited theory of psychological hedonism.

Freud and his followers, instead of attempting to define the human instincts, in the light of comparative psychology, instead of seeking what Freud now recognizes to be "so important for the establishment of a sound psychology", namely, "some approximate understanding of the common nature and possible differences of the instincts", have been content to postulate two groups of instincts, namely, sex instincts and ego instincts. They have been much concerned with the ontogenesis and phylogenesis of the former, but have been content to leave the latter group wholly undefined and unexplored. In addition Freud has occasionally made passing references to base instincts of cruelty, of destruction, or what not, without pausing further to define these.

The greatest need of present-day psychology is the incorporation with the hormic psychology, which has remained from Aristotle onward the soundest and most fruitful type of psychology, of all the valuable insight into human nature which the psychoanalytic movement and the genius of Freud have brought us. Freud's fundamental change of doctrine revealed in his "Beyond the Pleasure-Principle" is a most welcome step in this direction. It clears the way by repudiating a dogmatic error that has hitherto perverted all Freudian reasoning, and it sets the hormic principle solidly in the place of prime importance from which the pleasure-principle has been dethroned. But I do not believe that the mere regrouping of the sex instinct or instincts and the ego instincts as respectively life instincts and death instincts will serve to carry us appreciably nearer the much to be desired goal "some approximate understanding of the common nature and possible differences of the instincts" as the essential foundation for "the establishment of a sound psychology".

In conclusion I would repeat that progress of our knowledge of the instincts of man is not to be achieved by exclusive psychoanalytic study of nervously deranged patients. Valuable as such studies are, they need to be supplemented by and correlated with the results of wide comparative study of men and animals. When we take such a wide standpoint, does not the Freudian attempt to display such instinctive reactions as fear and curiosity as in some sense disguised expressions of the sex instinct, do not all such attempts appear as unbalanced and trivial exercises of minds narrowly confined to one small part of the whole vast field that invites the attention of the psychologist?

"INTERPRETING" A COMPULSION NEUROSIS

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I

IN THE romantic background of the Forest of Arden the sentimental melancholy of the banished Duke found "tongues in trees, books in the running brooks, sermons in stones, and good in everything". So Freud found "meanings" in things which had no "meaning" for others. Thus, for example, he found that our dreams, the idle and chaotic products of our sleeping minds, are expressive of our profoundest and most secret selves; that they are the imagined fulfilment of man's repressed (tabooed, unknown, and unknowable) criminality, brutality, and bestiality; that in them our unknown homosexual, perverse, and incestuous cravings find imaginary fulfilment in symbolic forms. He also found, or thought he found, that the trivialest "slips" or "accidents" in our waking conscious activities, such as slips of the tongue or pen, errors in hearing or seeing, accidentally breaking or dropping or losing or mislaying things, forgetting things we expect to remember, etc., are not really slips or accidents but are full of hidden meanings and give expression to trends, desires, motives, of which the individual is not aware at the time of the "lapse" and to which he objects to have a meaning assigned. (It must be confessed that the "interpretations" of these "slips" often have an artistic quality which is sheerly irresistible, especially to one who has not learned to subject these "analyses" to critical scientific inquiry. When this is done it invariably turns out that only rarely has the "lapse" a meaning, that, when such a meaning is present, the perpetrator of the lapse finds the meaning easily and usually either in the zone of marginal consciousness or in the vast and almost boundless realm of the foreconscious, and that most of the time the lapse admits of a perfectly satisfactory explanation which has no more to do with "meaning" than the splintering of a tree by the lightning.)

So too Freud maintains that the symptoms of the psychoneuroses (hysterias and compulsions) have a meaning, in fact, many meanings, that every such symptom is the product of "the Unconscious" and represents the symbolic enactment or fulfilment

of repressed (tabooed, unknown, and unknowable) desires of a normal, inverted, or perverted sexual nature. As Dr. Ernest Jones puts it (*Papers on Psycho-Analysis,* 1918, pp. 1–2): "he [Freud] made the remarkable and quite unexpected discovery that these [neurotic symptoms] have a definite psychological *meaning and purpose,* one, however, that is *totally unknown* to the patient. He found, further, that even when this meaning was laid bare by appropriate [?] methods, which practically *excluded the possibility of error* [!], at least of gross error, the revelation of it was not simply *surprising* to the patient, but *extremely distasteful;* so much so that the patient always strove as long as possible against accepting it, even in the face of *irrefragable evidence* [!]."[1]

How wholly unfounded Freud's theory is and what little justification there is for Dr. Jones' confident assertions will be shown in the following pages.

II

In his seventeenth lecture (*A General Introduction to Psychoanalysis,* 1920, pp. 222 ff.) Freud attempts to demonstrate to his hearers, a mixed audience of physicians and laymen, that a "symptom carries a meaning" just as errors and dreams do. For purpose of illustration he chooses an instance of a compulsive action indulged in for a long time by one of his female patients. He chooses a compulsive symptom, he tells us, because, owing to psychoanalytic research, compulsion neurosis has become more comprehensible and transparent than hysteria and we are in a better position to detect or recognize the patient's childish or ridiculous impulses and—most often—impulses bearing *"the terrifying aspects of temptations to fearful crimes"* from which *the patient flees in horror* and which he *denies even to himself but* to which he gives symbolic expression in his compulsive symptoms (thoughts or acts).

Freud introduces his "demonstration" with the remark that this particular analysis "cannot be replaced by anything more *complete"* and with a promise "to be *very explicit* and *to enter into every detail".* My comments on his analysis will show that he has *not* been explicit, that he has *not* gone into every detail, and that his fantastic explanation is the direct result of his failure to do so and his loyalty to a large body of unproved presupposi-

[1] Italics and matter in square brackets are mine throughout.—*S. A. T.*

tions, that, in other words, his interpretation is the product of autosuggestion and has no scientific value.

His analysis follows:

"A woman almost thirty years old—whom I might have been able to help if malicious mischance had not undone my work—suffered from very distressing compulsions; among other things, she executed the following remarkable compulsive action many times a day. She ran out of her room into an adjoining one, took up a position at a definite spot near a table in the center of the room, rang for her maid, gave her some trivial order or dismissed her without any order, and then ran back to her room. This was, of course, not a serious affliction but it is calculated to arouse curiosity. Its explanation was found *in the simplest and most unobjectionable manner, without any assistance* on my part. I cannot imagine myself guessing the meaning of her compulsive act or thinking of a suggestion tending to its interpretation. As often as I asked the patient 'Why do you do this? *what does it mean?*' she answered 'I do not know'. But one day, after *I had succeeded in overcoming a very deep-rooted scruple* ('ein grosses principielles Bedenken') of hers she suddenly understood ('wurde wissend') and *narrated what appertained to the compulsive act.* More than ten years ago she had married *a very much older man* who proved to be impotent on the bridal night. Innumerable times during the night *he ran from his room into hers* to repeat the attempt, but each time in vain. In the morning he said vexatiously: 'It is enough to make one ashamed before the maid when she makes the bed', seized a bottle of red ink which happened to be in the room and poured its contents on the sheet, but, as it happened, not on a spot where such a stain would have been justifiable.

"At first I did not understand what this memory had to do with the compulsive act in question, for in the narrative and in the symptom I found agreement only in the fact that a person runs out of one room into another, and possibly also in *the appearance of the maid.* Thereupon the patient led me to the table in the adjoining room and *let me discover* ('liess mich . . . entdecken') *a large spot on the tablecloth.* She also explained that she placed herself at the table in such a way that the summoned maid *could not help seeing the stain.* Now it was no longer possible to doubt the intimate connection between *the scene following the bridal night* and her present compulsion, though there were still a number of things to be learned about it.

"It is, in the first place, clear that *the patient identifies herself with her husband;* she plays him by imitating his running from one room into another. To maintain the parallel we must also concede that she *replaces the bed and the sheet by a table and a tablecloth.* This might seem arbitrary on our part, but we must not omit to profit from our study of dream symbolism. In dreams, too, *very often* a table appears which must be interpreted as a bed. 'Bed and board' constitute marriage; one may, therefore, easily represent the other [*i.e.,* a bed may stand for a table, and *vice versa*].

"We may consider it *proved,* then, that *the compulsive action* has a meaning; it seems to represent or *repeat that significant scene.* But we are under no necessity to stop with this seeming; if we examine the relations between the two [*i.e.,* the compulsive act and the scene] more thoroughly we shall probably be enlightened concerning something more far-reaching, to wit: the *purpose* of the compulsive action. *The essence ('der Kern') of the compulsive action* is evidently the summoning of the maid before whom she displays the stain in opposition to her husband's remark: 'It is enough to make one ashamed before the maid.' Now he—whose rôle she plays—*is not ashamed* before the girl, and consequently *the stain is in the right spot.* We see, then, that she has not merely repeated the scene but continued it as well as *corrected it,* 'turned it to the good'. But therewith she also corrects the other matter, that which had been so distressing *that night* and *had made the use of the red ink necessary,* to wit: the husband's impotence. The compulsive action therefore proclaims: 'No, it is not true; he did not have to be ashamed before the maid; he was not impotent.' After the manner of dreams, she depicts the fulfilment of a wish by means of a present action; she *labors to help her husband over that unfortunate incident.*

"All else that I might tell you about this woman would bear this out; more correctly speaking, all else that I know about her points to this interpretation of an otherwise incomprehensible compulsion. The woman has for years been living apart from her husband and cannot bring herself to the point of carrying out her desire to initiate legal proceedings for a divorce. But she is by no means free from him; *something compels her to be true to him;* she shuts herself up from the world so as *to secure herself against temptation;* in her fantasies she excuses and idealizes him. The great secret of her affliction is that by means of it *she shields her husband from malicious gossip, justifies her separation from him,*

and makes a comfortable life possible for him away from her. . . .

"I shall be glad to have you dwell upon this example, as it combines conditions one cannot as a rule expect to find in other cases. *The interpretation of the symptoms was found by the patient herself* in one flash, *without the analyst's suggestion or interference,* and it came about as the result of a reference to an experience which did not, as usually, relate to a forgotten period in childhood, but to the adult life of the patient in whose memory it had remained unobliterated.

"All the objections which critics ordinarily bring against our interpretation of symptoms fail in this case. Of course, we are not always so fortunate.

"One thing more! Have you not noticed that this *apparently insignificant compulsive act introduced us to the patient's intimate life?* There is nothing much more intimate for a woman to relate than the story of her bridal night. And is it *accidental* and without further significance that we happened to come on the intimacies of her sexual life?"

III

This, then, is an analysis which challenges and defies criticism, from which the objections of the critics "roll off" as water from a duck's back, and which "proves" and "demonstrates" that the symptoms in the psychoneuroses have a "meaning", "serve a purpose", and are the product of the Unconscious! Careful examination of this "analysis" will show, however, that *the data furnished the reader do not prove what is claimed for them, that the investigation was not conducted in a scientific manner, that the investigator took too much for granted, that he failed to inform himself as to essential details, that he focused his attention on a few selected facts and distorted their relationship to one another, that he deluded himself into thinking he had enough facts to prove what he would have liked to discover, and that to satisfy himself that the symptom meant what he wished it to mean he resorted to the arbitrary assumption that selected elements in the symptom had symbolic significance and then gave these an arbitrary interpretation*—a technique by which, all will admit, anything may be made to "prove" anything one sets out to prove.

If the analysis were as complete and explicit as we were promised it would be, we should have been informed about a great many matters concerning which we are not given the

slightest hint. In the determination of so complex a problem as the possible "meaning" of the symptom selected for analysis we must know, for example, how long the woman had her neurosis, how long she had this particular symptom (we have been told that she had others too), when and under what circumstances this particular symptom first manifested itself, how long after her marriage, how long after her separation from her husband, what symptoms had preceded this one, etc. All will admit that answers to these questions are absolutely essential to any understanding of the patient and the evolution of her neurosis, and yet Professor Freud gives us not the slightest hints towards an answer to any of these questions, notwithstanding his assurance that his analysis is complete, explicit, and minutely detailed! The importance of such data is apparent to all who have learned that specific individual symptoms are invariably intimately related to the circumstances under which they have come into being.

Before we can accept the above interpretation we have every right to be informed why the girl (she was less than twenty at the time) married a man so much older than herself that he was impotent; why and how long she continued to live with him; why and when they finally separated; why she could not bring herself to initiating divorce proceedings; why she did not proceed (more simply and logically) to have her marriage annulled; why she was or should have been so concerned about shielding the old man from malicious gossip; why she assumed that he would be subjected to gossip; why she cared more about the old man's comfort than her own. Surely no one will say that a complete and explicit analysis should not have enlightened us on these matters or that information along these lines might not throw important light on the possible significance of the woman's symptoms.

Several times in his analysis Freud stresses the point that the symptom's explanation was found "without any assistance" from him, without any suggestion from him or interference on his part. But surely he knows, no one better, that there is not an individual consulting him who does not know in advance that he attributes all the neuroses to disturbances in the sexual sphere, that his patients are expected to lay their sexual lives bare in all their manifold details, and that he interprets all symptoms in terms of sex and as sexual equivalents? Has he not himself told us in one of his essays of a young girl telling him on the occasion of her first consultation that she knew he would tell her she was in love with her papa and was jealous of her mama? If he had told us

that the woman knew nothing of his theories we would accept his statement that he had not influenced her in the interpretation of her symptom. But he does not tell us this, and we are therefore warranted in assuming, with a high degree of probability, that she did know his theories and what he expected her to "discover". If this is not suggestion, what is it? That all psychoanalysts exert a certain "influence" on their patients is inevitable from the fact that they always devote the first hour or two to informing their patients concerning the essentials of the Freudian theories. (*Cf.* S. E. Jelliffe, *The Technique of Psycho-analysis.*) In the light of this fact it must be clear that we should also have been told how long the patient had been under treatment and how much she had read or been told about psychoanalysis before she "saw the light" with respect to this symptom. But Freud is as silent on these subjects as on many others no less important.

In this connection would it not have been of some importance for a critical audience to know—it certainly would for an audience psychologically trained—what the analyst and his patient had been discussing just prior to her recollection of the tragedy of the bridal night? Freud tells us, with characteristic vagueness, that he had just "succeeded in overcoming a very deep-rooted scruple of hers". (I may add that Boni & Liveright's translator of the *Lectures on Psycho-Analysis* renders the words "ein grosses principielles Bedenken" by "a grave ethical doubt" instead of "a very deep-rooted scruple".) To me the statement suggests only that Freud had overcome the woman's scruples on the subject of discussing her sexual life. Friends whom I have asked about it agree with me in thus interpreting the sentence. If, then, sex was the topic of discussion, would anything be more natural and inevitable than that the woman should have recited an important and humiliating item in her sexual experiences? And, furthermore, if he was persuading her to speak of her sexual experiences in connection with her symptoms, could he honestly say that she discovered the meaning of the symptom without his influence or interference?

Somewhat ambiguously the analyst then tells us that the patient led him into an adjoining room—the "treatment" was evidently conducted in the patient's home—and "let him discover" a spot on the tablecloth. Inasmuch as he does not say she pointed out the spot we are permitted to wonder how she "let him discover" it and what would have happened if he had not noticed the spot. Would the analysis have stopped there? Would

she have showed him the spot? How could the analysis have been accomplished if the patient had been treated at the analyst's home? Why was it necessary for her to lead him to the spot? Did she intend him to discover the stain? And why, having led him to it, didn't she point it out? The answer to some of these questions is really simple. The woman was reënacting the scene with the maid; inasmuch as she did not draw the maid's attention to the stain, she could not call his attention to it. From these considerations there ensues a grave objection to Freud's explanation of the compulsive action: if the maid did not notice the stain, the patient's wish to have the maid think that the bridal night had gone off well was *not* fulfilled. If that had been the woman's wish, as Freud tells us it was, she should have made sure that the maid saw the stain. But there is nothing in Freud's account to show that the maid ever saw it—she certainly did nothing about it. From what we hear women say about maids we may rest assured that in all probability the maid was blind to that spot.

While on this subject let us call the reader's attention to the important fact that the color of the tablecloth has not been mentioned and that the size, color, and location of the stain are wholly unknown to us. And yet everyone must know that these factors would materially influence the maid in seeing or not seeing the spot on the tablecloth. Seeing that the crestfallen husband had spilled red ink on a white sheet, we are justified in asking whether the disappointed wife had spilled something red or only gravy on a white or colored tablecloth.

This leads us further to ask whether the woman always used the same tablecloth, day after day, year in and year out, or whether she took a fresh tablecloth every time she summoned the maid. And did she make a fresh spot every time she rang for the maid? Did she try to learn whether the maid saw the spot? And how did the woman justify her conduct to herself? All neurotics find some justification or rationalization for their symptoms. What explanation did this woman give herself for her strange conduct? And how long did the maid put up with being summoned many times a day and being dismissed with a trivial order ar no order at all? The more one studies this the more incredible the whole thing appears.

And one can't help wondering why this woman's maid never quit her job and never thought of removing the spotted tablecloth? Other maids might after a while have refused to answer the mis-

tress' bell, thus compelling her to evolve or develop a different symptom, and we should then have been without a most artistic and typical example of psychoanalytic practice.

Let us now turn our attention to the fateful bridal night.

Professor Freud tells us that the husband was impotent, and he leads us to infer that the cause of the impotence was the man's age. But there is surely something wrong here. We know that only extreme old age can cause physical impotence. Try as we may, we cannot conceive of a girl not yet twenty years old marrying a man so old that he could not discharge his marital obligation even once on his bridal night. Had he been as feeble as all that he could not have (and would not have had reason to) run into her room innumerable times that night.*

At this point of our inquiry certain other important questions suggest themselves. Was the man impotent prior to his marriage? Did the impotence continue after the bridal night? Was that the cause of the separation? What shape did his impotence take? Answers to these questions are of the utmost importance in determining the cause or causes of the wife's neurosis. If she was not of a passionate disposition, had had no sexual experiences, and her husband did not excite her manually or otherwise, she would probably not have developed a neurosis unless she had other problems or conflicts of an insoluble nature. If he was potent after the marriage, it is incredible that she would develop a neurotic compulsion about the mishaps of the bridal night.

We must admit, too, that we cannot believe that a man, even an old man, who hopes to be able to cohabit with his young bride, would sleep in a room adjoining hers on their wedding night. And even if such a thing were imaginable, it is beyond the reaches of our soul that such a man, experiencing a loss of libido in transit, would return to his room over and over again every time it happened. One is almost tempted to believe that the analyst was trifled with by his patient.

It is not impertinent to inquire why the lady subsequently took the matter so seriously. As far as this explicit and detailed analysis tells us, she does not seem to have been affected in the slightest degree by her husband's ill-luck. And yet we are ex-

* In this connection we ought to be informed too whether the woman really "ran" into the adjoining room, or only walked in. That a man fearing impotence might run under the given circumstances is undeniable; but if the wife nullifies the failure, why should she run? And if she did run, what conscious explanation did she give herself for so doing?

pected to believe that years later she developed a symptom which meant a nullification of her (temporarily?) preserved chastity!

Though we have already referred to the analyst's failure to mention the color of the stain on the tablecloth, we must refer to the matter again, but this time from a different angle. The chagrined husband, we are told, poured a bottle of red ink on the sheet. The thought in his mind, the analyst would have us think, was that the maid, noticing the absence of a blood stain on the sheet, would infer that the husband had lacked the virility to discharge his function. Professor Freud, wholly without warrant, evidently attributes the same thought to his patient in her neurosis; but he gives no reason why she should have thought that rather than that the maid, in the manner of maids, would think that the bride had not been a virgin. The latter thought, I submit, would have been much more humiliating to the bride than the former. Had the husband really been anxious about what the maid would think of him he would surely have had acumen enough to stain the sheet with egg-albumin (which would have been easily obtainable). The color of the stain on the tablecloth is therefore a matter of considerable importance from at least two points of view, and yet the analyst says nothing about it, notwithstanding his promise to be explicit and to spare no details.[2]

We might of course also inquire why this woman should have imitated her unhappy husband in worrying about what the maid would think. A nice woman would have been careful to keep evidences of the happenings of the bridal night from the prying eyes of a curious maid. This consideration alone suffices to make me doubt that there was any relationship between the symptom and the memorable bridal night. If Dr. Freud's patient was the kind of woman who takes her maids into her confidence as to her intimate affairs, or cares about what her maids think, that is something about which his readers have a right to be informed when it is a matter of establishing a new and extremely important psychological principle.

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[2] Other pertinent questions suggest themselves at this point: Was the husband really such a fool as to spill a whole bottle of ink on the sheet? If so, how could it be on the wrong spot? What did they do the next morning? What, if anything, did they say to the maid? Did they conceal the ink-stained sheet? What did they do with the rest of the bedding? How did he explain his mishap to his wife? How did she take his failure? These are important questions because the answer to any one of them might be the basis for hysterical symptoms.

IV

After these preliminary considerations we may turn our attention to the interpretation of the compulsive action.

In the first place, it will be noticed that Professor Freud assures us more than once that the patient found the interpretation wholly without his assistance and without any suggestion from him. But when we reread his account of the matter we are amazed to discover that there is no correspondence between his statement and the facts. His patient did *not* interpret the symptom; she did not connect her story with her compulsive act; she said nothing about identifying herself with her husband; (neither she nor her analyst gives us the slightest hint why she should have identified herself with her husband); she found no wish fulfilment in her compulsive action; she said not a syllable about nullifying her impotent husband's failure to deflower her; there is not the slightest intimation from her that she identified the table with the bed or that she had the slightest concern for her husband's estimation in the maid's mind. Wherein, then, did *she* interpret her curious symptom? Is it not perfectly obvious that the interpretation is wholly the product of Professor Freud's fantasy? and that he gets his results by arbitrarily identifying one thing with another (the woman with her husband, the table with the bed) till he gets something that complies with his presuppositions (*e.g.*, that the symptom must have a sexual significance, that it must be a wish fulfilment, that it must be the opposite of the patient's conscious desires, etc.)?

We recollect, of course, that the analyst tells us that after he had overcome his patient's scruples about a certain matter, she "narrated what appertained to the compulsive act". It is, to say the least, remarkable that here too Freud's language is strangely obscure. He says, in the original German, that the woman "erzählte was zur Zwangshandlung gehörte", (literally, "narrated what appertained to the compulsive act"). Boni & Liveright's translator renders the words incorrectly thus: "related the history of the compulsive act"—and this is in all likelihood the meaning that the lecturer wanted to convey to his audience, but which he refrained from saying directly because he would not tell a deliberate untruth. What he got from the woman was *not* the history or meaning of her compulsion; all she did was to tell him—no doubt, among other things—after he seems to have prevailed on her to discuss her sexual life, a striking and memorable

incident in her marital life. She made no reference to the compulsive act. What she told him may have been no more than her way of telling him that she was still a *virgo intacta*. But the analyst at once saw, or thought he saw, a connection between the thing (or one of the things) narrated and the description of the compulsive act: in both someone runs from one room into another and the word "maid" figures in both. Then the woman led him to the table in the next room and "let him discover" a "spot" on the tablecloth. That was enough for him: repeated going from one room into another, maid, spot. To make the analogy more apparent he arbitrarily substituted the woman for her husband (though the latter had *not* summoned the maid) and translated "table" by "bed"—because in dreams a table sometimes means a bed. If under such circumstances one may say that the woman herself interpreted the symptom, words have lost their meaning. And yet the analyst tells us that the explanation was found in "the simplest and most unobjectionable manner" and "without any assistance" on his part!

Freud also speaks of the "appearance of the maid" as an element occurring "in the narrative and in the symptom". But the reader will recollect that in the narrative the maid did *not* appear, that the maid was only mentioned by the husband as someone who might make him feel ashamed. It is obvious, of course, that the analyst's distortion was a necessary one for his interpretation. Doesn't it seem that the maid was introduced into "the scene following the bridal night" because she is an essential element in the compulsive symptom?

After the woman had told her analyst about the disappointments of the bridal night, he was unable to "understand what this memory had to do with the compulsive act". But why did it even occur to him that there was any connection between the two things? She had many other symptoms; she had spoken of other matters. Why didn't he think of some other symptom in connection with her "memory"?

How far-fetched and improbable the assumption is of any relationship between the compulsion and the bridal night is fairly clear from a consideration of the differences between the two; the latter occurred by day; the man entered the room in a state of sexual excitation; the spot was on the cloth *before* the woman entered the room; the husband returned to the other room crestfallen and disappointed; his errand was in vain; he made the spot on the sheet himself (it is a noteworthy circumstance that the

analyst does not tell us who stained the tablecloth); his mind was on the act of cohabitation; and so forth. The points of difference, in other words, are so striking that common sense refuses to identify one scene with the other. And yet on the strength of only two points of resemblance between the two scenes—or three, if we grant the analyst the appearance of the maid—Freud says it was "no longer possible to doubt an intimate connection" between them!

Because a table sometimes means a bed in a dream is no justification for giving the table in the patient's symptom that meaning. In the first place, there is no logical reason for giving the table a symbolic meaning any more than for giving some other element in the symptom a symbolic meaning. Thus, for example, the woman's "going" might, if one so desired, be interpreted as meaning "coitus" because of an etymological connection between "coitus" and "coire" (to go with). Or, again, the "spot" might be interpreted as a symbol for the feminine parts. This kind of nonsense is surely not a whit more arbitrary or less absurd than Freud's identification of a table in a symptom with a bed because "bed and board" are a synonym for marriage.

In the second place, a table does not symbolize only a bed. We all know that the word "table" may symbolize food, a flat surface, the palm of the hand, a group of people, etc. And yet Freud selects "table"="bed" for his interpretation. And he calls that "unobjectionable" technique, technique which excludes the possibility of error![2a]

The woman, we are told, is not ashamed before the maid, and that means that the stain is on the right spot. But this will not do. A perpetual stain on the tablecloth is something the woman *should* be ashamed of. And if she is not ashamed of her slovenliness we want to know why she is not and how she accounts for it to the maid. And if she *is* ashamed when she shows the maid the stain, then the analyst's interpretation is disproved.

We are told, too, that the patient picked good from bad by letting the maid see that something had happened. In the absence of more detailed information about this woman we may be permitted to doubt that she was so vulgar as to be willing to take her maid into her confidence about such intimate matters as the happenings on her bridal night. And, besides, if she did not

[2a] Besides, even assuming that to the patient a table sometimes symbolized a bed, etc., how do we know it does so in this instance? And if a table does not mean a bed to the maid, of what value is the symptom as a communication to her?

make the stain herself in her assumed rôle of husband, the whole thing means nothing. If she *did* make the stain, we are entitled to know what her conscious motives were. If the husband had been potent that night would he have summoned the maid next day to show her the evidence?

And what is the upshot of the whole business? That "unconsciously" she labors to create a situation which would imply that her husband had not been impotent on the bridal night. (Again we ask "and what of the other nights?") And why is this so "extremely distasteful" to the woman that she had to repress it into her Unconscious and give it expression in a symbolic shape? Evidently either Freud's interpretation is wrong or Jones' introductory statement does not fit the neuroses, notwithstanding the "irrefragable evidence" on which it is based. What is there about the woman's alleged wish that she should want to "flee from it in horror" or that it should be classed with "temptations to fearful crimes"?

In the course of his lecture Professor Freud asks his hearers whether they had noticed the remarkable fact that the analysis led to the patient's most intimate (*i.e.*, sexual) life. He has evidently forgotten that he had induced his patient to speak of her sexual life by overcoming one of her deep-rooted scruples. And does not every patient know that Dr. Freud expects all his patients to discuss their sexual life at length? And is it not obvious that if a patient is going to speak of everything that comes into her head she is bound sooner or later to speak of her sexual life?

Finally it must be pointed out that the analyst's interpretation of his patient's symptom—which, by the way, we must remember, was not cured—is not the only interpretation that can be wrung out of the symptom by the psychoanalytic technique. Thus, for example, we might interpret the compulsive act (1) as a manifestation of a desire to prove to the maid that she *was* a virgin on her bridal night (not that her husband was potent), or (2) that she indulged in coitus frequently (as shown by the many stained sheets), or (3) that she was making homosexual advances to the maid (by summoning her, showing her the "spot" and standing by the table [=lying on the bed]), or (4) that she was unconsciously saying to the maid: "Pity me! see what a wretched marriage I have made! This old dotard has to stain the sheets with ink to give the impression of potency!", or (5) that she thus proclaimed to the world (through the maid) her husband's unfit-

ness and her justification for not living with him, and so forth. There is practically no limit to this kind of foolery. For aught we know to the contrary, the symptom may have meant only a knowledge on the woman's part that the tablecloth was so stained that it ought to be removed and washed but that she lacked the courage to call the maid's attention to the matter directly, or that she wished to see how long it would take the maid to discover that the cloth was stained.[3]

We have also been told that "something" compels the woman "to be true" to her absent husband, that she has "shut herself up from the world so as to secure herself from temptation", that by means of her neurosis "she shields her husband from malicious gossip and justifies her separation from him", etc. But as these assertions are evidently only the analyst's interpretations and probably no better founded than his interpretation of the compulsive act, we may dismiss them without further comment.

It is the wilfulness, arbitrariness, and capriciousness of the Freudian technique and interpretations appearing in every instance of an "analysis" submitted to critical tests that prove orthodox psychoanalysis to be nothing more than a cleverly conceived therapeutic system which has no more scientific validity than Christian Science.

[3] It might also be maintained psychoanalytically that the symptom was the woman's way of showing the maid that she had been deflowered, that all had gone well; and in that case it would be wholly unnecessary to assume that in the symptom the woman stands for her husband.

THE PSYCHOLOGY OF BELIEF

A Study of Its Emotional and Volitional Determinants

By FREDERICK HANSEN LUND

I. INTRODUCTION

PSYCHOLOGY has been studying mental and physical functions in isolated aspects. This is an inevitable result of the analytic method and the structuralism dominating the older psychology. Among the sciences psychology has formed no exception with respect to the ruling obsession for knowledge of phenomena in its most elementary form. But in the psychological sphere more than in any other significant reality is not to be found in the elements. Within the domain of psychology the elements are derived through artificial isolation, and complex mental states are inexplicable as mere products of the presumed elements.

Psychology must depend ultimately for its advance and its usefulness, not upon a study of "structural units" or "elementary traits", but upon an understanding of significant behavior and the causal principles fundamental to it. No great contribution to society would be made by psychology if it never got beyond structuralism to the study of the human individual as such, and especially to a study of those traits which give form to his character and direct his motivations. Only few and rather hesitating attempts by way of experimental procedure have been made with a view to determine the most significant aspects of human personality as found on the emotional and volitional side. The present study has been toward that end inasmuch as its chief interests center about the affective processes conditioning individual belief and social sanction.

FIELDS FOR WHICH BELIEF IS OF SPECIAL INTEREST

Belief, complex as it is, plays a rôle of great importance within numerous fields. It is of special concern to all whose success depends upon an understanding of human interests and motivations. The advertiser, salesman, journalist, author, politician, minister, and lawyer, cannot afford to be ignorant of the nature and conditions of belief. "What interests people?" is the question dominating these fields, but it might be more expressly

stated as, "What do people want?" or, "What do people believe?" To couple our "wants" or desires with our beliefs may appear as an extravagant usage, but, as we shall see, they are not distinct and in fact overlap considerably.

The public speaker, orator, and politician, probably without ever having really analyzed human motivations, will be aware that their capacity to sway an audience depends in each case upon the nature of their appeals, and whether these conflict with primary passions and preconceived notions. Such considerations are among the most important in determining the devises by which the artful speaker gains an inroad for the ideas he wishes to disseminate.

The advertiser, skilled in his art, takes due account of what people believe or do not believe, and guards against expenditure on advertisements which conflict with general opinion. He is careful not to presume too much on the credulity of the public while gaining favor for his commodity through appeals to their interests, prejudices, likes and dislikes.

The newspaperman and the novelist depend for their hearing on much the same factors. The news article is made entertaining as much through confirming popular bias as through its sensational and spectacular nature. The novel presents human nature at its best and at its worst, thus allowing for the greatest interplay of emotions, and in this idealized form makes possible the reader's realization of ideas, desires, and inner cravings, gained through more concrete experience.

In so far as this characterization is correct we should look to these sources mainly for a reflection of ourselves. But inasmuch as the expression of these activities is the result of a great deal of practical experience they assure us of the importance of belief and the extent to which it is conditioned by emotional and volitional factors. The existence of non-rational factors influencing belief has, I think, been generally recognized, but there is great need for quantitative determination of their relation. Curiously enough none of the experimental work which has been done so far on belief has given any attention to its affective determinants.

HISTORICAL BACKGROUND

The earlier writers on the subject of belief, confidence, or assurance, as it is variously called, are ably reviewed by Lindsay[1]

[1] Lindsay, J. "Psychology of Belief," Arch. für Syst. Phil. 1910, 16, 293-309.

and by Okabe,[2] and need not be considered here except as problems under consideration may call for it.

Only a few experiments on the subject have been carried out. The results of the most important of these may be summarized briefly.

Fullerton and Cattell, in 1892, found that "some observers are not confident unless they are, in fact, right; while others are often confident when they are wrong".[3]

Sumner, in 1898, working with 25 selected statements, and using the order of merit method, found that his "subjects considered it possible to arrange their beliefs in a graded series".[4]

Okabe, in 1910, working with six experienced observers, concludes on the basis of their introspections that: "(1) Belief and disbelief are consciousness of the same kind. (2) The certainty-uncertainty consciousness closely resembles that of belief-disbelief, but is in general more strongly affective. Certainty is pleasant; uncertainty, doubt, is unpleasant. (3) Belief-disbelief consciousness except in the form of quasi-mechanical acceptance or rejection is not of common occurrence, and at first is not of common occurrence under experimental conditions".[5]

Henmon, in 1911, experimenting on the relation between the time of a judgment and its accuracy obtained no significant correlation between the two.[6]

Williamson, in 1915, used the order of merit method and paired comparisons with a view to determine the relation between the degree of confidence in one's judgment and the objective difference in the material judged. His correlations relative to these factors ranged from .70 to .96.[7]

Trow, in 1923, worked along the same line as Williamson. His subjects were asked to judge which was the heavier of two weights, or the longer of two lines, etc. After the subject had passed his judgment he was to indicate the strength of his confidence in his judgment by referring it to a scale of four degrees. Trow's conclusions confirm those of Williamson that correctness

[2] Okabe, T. "An Experimental Study of Belief," *Am. J. of Psychol.*, 1910, 21, 563–596.

[3] Fullerton and Cattell. "On the Perception of Small Differences." *Publ. of the Univ. of Penn.*, 1892, 2.

[4] Sumner, F. B. "A Statistical Study of Belief," *Psych. Rev.*, 1898, 5, 616–631.

[5] Op. cit., p. 596.

[6] Henmon, V. A. C. "The Relation of the Time of a Judgment to its Accuracy," *Psych. Rev.*, 1911, 18, 186–201.

[7] Williamson, G. F. "Individual Differences in Belief, Measured and Expressed by Degrees of Confidence." *J. of Phil. Psychol., etc.*, 1915, 12, 127–137.

varies with confidence. He also found that the most confident type of individual as judged from the total confidence scores was no more likely to be right than the others.[8]

Poffenberger, in 1923, found from his experiments on advertising that rational considerations and understanding of an advertisement were not necessary to establish belief in its content; also, that the truth of what is presented in an advertisement does not insure public acceptance of it.[9]

The questions relative to belief which the present study has attempted to answer are as follows:

I. Does belief have an emotional content, and is it conditioned to any extent by the presence of emotional factors?

In determining the first point, groups of students were asked to rate a set of propositions, first for belief-strength, and, in a second rating, to indicate the extent to which they *desired* that the proposition's content were true or not true. Since the ratings were made on scales of comparable steps or degrees, it was possible to correlate the two ratings; and the coefficients obtained were considered as measures of the relation existing between belief and desire for the range of materal used. On the second point, as to the possibility of emotional factors existing as *determining* influences of belief, the procedure was directed toward establishing the further relation existing between the ratings for belief and desire and more objective measures of the certainty of the propositions' content.

II. Is the rational principle conceived as the most valid and ideal among belief-determinants? How would individuals rate themselves as over against their fellows on a rationality scale?

A list of belief-determinants was drawn up on an empirical basis, and subjects were asked to arrange them in order of merit as to their dignity and validity as determinants of belief. If the rational principle exists as an ideal the order resulting from such an arrangement should be determined by the amount of rationality assumed by each belief-determinant. Since the results proved this to be the case to a marked degree, the list was considered as a rationality scale (each determinant being assigned its position in accordance with its place in the arrangement for merit) and was used in the second part of the experiment in which subjects rated first themselves and then "the average college student", indi-

[8] Trow, W. C. "The Psychology of Confidence." *Archives of Psychology*, 67, 1923.
[9] Poffenberger, A. T. "The Conditions of Belief in Advertising." *J. of Applied Psychol.*, 1923. Vol. 7, pp. 1–9.

cating the extent to which the rational principle operated as a belief-determinant in the case of each proposition.

III. Are there any emotional and volitional factors present to make *order* a significant factor in persuasion?

In the investigation made on this point, groups of subjects were given pro and con discussions on a given proposition, after they had first rated this proposition for belief strength. One group would be given a positive discussion first and a negative discussion afterwards, while another group would be given the same discussions in reversed order. After each reading a rating for belief was required of each subject. It was believed that if order was a factor there would be a difference in the extent to which the subjects could be swayed, depending on which discussion they had had first.

IV. What, finally, can be said of the nature of belief and its relation to the concepts of knowledge and opinion?

The definition of these terms and the relation existing between them was determined by the results obtained from the various rating schemes used throughout the experiment.

A more detailed description of the method and procedure employed in each part of the experiment will be given in its appropriate connection.

METHOD AND PROCEDURE

For my general method and procedure I am indebted in a large measure to the suggestions of Prof. H. L. Hollingworth. In carrying out the experiment my acknowledgments are especially to Prof. W. Hyde and Prof. E. L. Hinman of Nebraska University; Dr. H. E. Garrett, Mr. G. T. Renner, and Mr. L. Wolfanger, of Columbia University; and to the students of Barnard College, Columbia College, and Nebraska University, who served as subjects.

A set of thirty propositions were drawn up, selected from the fields of religion, ethics, politics, and science. Six were chosen from each of these fields with six additional ones not readily classifiable. All, however, had definite bearing upon topics of common interest.

Scales were constructed for belief, certainty, desire, and evidence, each of which had ten steps or degrees, but in the case of the first three actually allowing for twice that number of ratings, since it became necessary to have plus and minus ratings, plus ratings for belief and minus ratings for disbelief. For a better

understanding of these scales they are given here with their directions and the list of propositions.

1. Was Lincoln an honest and upright man?
2. Is a democracy the best form of government?
3. Does a black cat crossing your path cause bad luck?
4. Were the higher forms of life derived from the lower forms through a gradual process of evolutionary growth?
5. Will the death penalty for murder always be held justifiable among civilized peoples?
6. Did the whale swallow Jonah?
7. Do molecules exist?
8. Did Shakespeare write "The Merchant of Venice"?
9. Is Christianity losing its influence in this country?
10. Is the earth practically round?
11. Do only the good die young?
12. Is slander wrong?
13. Are there other beings besides myself?
14. Will monogamous marriage continue to be the only socially accepted relation between the sexes?
15. Will our Republic continue to exist a hundred years from now?
16. Should all men have equal political rights?
17. Do air vibrations constitute the stimulus for hearing?
18. Does death end personal existence?
19. Is morality a man-made institution?
20. Do two plus two equal four?
21. Is a man's conduct determined entirely by his heredity and environment?
22. Did the world come into existence through the creative act of a divine being?
23. Does the earth travel around the sun?
24. Is the protective tariff a wise policy for the United States?
25. Do animals have feelings similar to our own?
26. Is the sun the source of light?
27. Do any landscape paintings yield as much satisfaction as the finest natural scenery?
28. Is the Golden Rule a practicable concept in business relations?
29. Will traffic in liquor ever be entirely abandoned?
30. Did the dinosaur ever exist?

PART I.

The "scale of belief" is to be used as an index to the strength of your belief, or as a standard measure by which you rate the propositions in accordance with the degree to which you feel inclined to affirm or deny their validity. It is not a question of what you know, or of what you can be sure, but what you *think* or *believe*. Consider each proposition carefully and after locating its position on the scale indicate this position by noting its plus or minus number in the left hand margin.

Scale of Belief:

10. 9.	Belief allowing for no doubt.
8. 7. 6.	Fairly strong belief.
5. 4. 3.	Slight belief—an element of doubt.
2. 1. 0.	Absence of both belief and disbelief.
— 1. — 2. — 3.	Somewhat inclined toward disbelief.
— 4.	

— 5.
— 6. } Fairly strong disbelief.
— 7.
— 8.
— 9. } Disbelief allowing for no doubt.
—10.

PART II.

In applying the "scale of certainty" the thing you are to keep in mind is not the strength of your belief but the *certainty* with which you can affirm or deny a given proposition. How sure are you? How can you know? Indicate your estimates as in experiment I.

Scale of Certainty:

10. 9. 8.	Very certain	that you can affirm it
7. 6. 5.	Fairly certain	
4. 3. 2.	A degree of certainty—not sure	
1. 0.	Uncertainty—chance	
— 1. — 2. — 3. — 4.	A degree of certainty—not sure	that you can deny it.
— 5. — 6. — 7.	Fairly certain	
— 8. — 9. —10.	Very certain	

PART III.

The "scale of desire" holds no relation either to the truth of the proposition or the certainty you may feel in regard to it, but is to be applied solely with reference to the emotional content of the proposition. One may regard a proposition with indifference or with intense interest quite irrespective of its validity. As before, indicate your rating in the margin but bear in mind that it is to be a measure of the proposition's *desirability* or *undesirability*—your *wish* that it were true or not true.

Scale of Desire:

10. 9. 8. } Highly desirable.
7. 6. 5. } Quite desirable.
4. 3. 2. } Somewhat desirable.
1. 0. } Indifferent.
— 1. — 2. — 3. — 4. } Somewhat undesirable.

— 5.
— 6. } Quite undesirable.
— 7.
— 8.
— 9. } Highly undesirable.
—10.

PART IV.

Construct two columns, one for *evidence* and the other for *opinion*, and assign each proposition a percentage value in each column. In assigning this value consider the extent to which the proposition's content is within the bounds of demonstrable and indisputable fact and the extent to which it remains a matter of opinion.

Proposition	Indisputable Evidence	Matter of Opinion
I.	25	75

The method and procedure in connection with the testing on the rationality scale, and the testing for the law of primacy in persuasion will be presented with the discussion of the results of these tests.

No instructions were given orally and in no case were the subjects informed beforehand as to the nature of the tests. Each subject was provided with a list of the propositions and a sheet of directions for the part of the experiment in hand.

II. BELIEF AND DESIRE

That belief is conditioned by desire, or at least holds a very definite relation to certain emotional constituents, is frequently recognized. In its most popular form the idea occurs in the adage, "the wish is father to the thought".

Expressions on the subject are not wanting in scientific literature, though no backing has been available from experimental work. James' "Will to Believe"[10] is perhaps the best known of the writings contending for the presence of irrational factors influencing our belief-attitudes. Sumner declares, that "if there is one thing with which the student of the subject is impressed, it is with how small a fraction of our beliefs arise in the first instance through reason, or, having arisen, are maintained by it. *One believes most vividly what appeals to one's interests most strongly,* although the thing believed may take a low place in the

[10] James, Wm. "Will to Believe." Houghton Mifflin Co. 1895.

scale of certainty.[11] Poffenberger, in discussing conditions of belief in advertising, says, "*we tend to believe what arouses our desires, our fears, and our emotions generally.* . . . In strong emotion we might find the condition for belief in the bargain counter. . . . If one really wants a certain suit of clothes or an automobile which costs more than he can pay, he may honestly believe he is making an economical purchase."[12]

THE DETERMINATION OF AN OBJECTIVE MEASURE

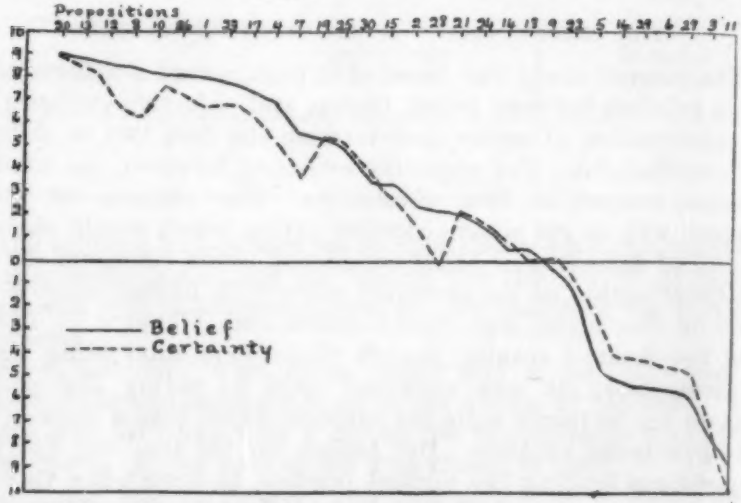
The present study was directed in part toward a determination of the relation between belief, desire, and objective evidence. In the construction of scales to determine the first two no difficulty was encountered. For objective evidence, however, no adequate measure seemed at first obtainable. The purpose of such a measure was to get some objective rating which would show the amount of deflection of belief occasioned by its emotional content. The construction of the certainty scale with units comparable to those on the belief and desire scales (see Chapter I) did not yield the desired results, though these were interesting enough in themselves. It was supposed that in rating the propositions on the certainty scale the subjects would take a more critical and impersonal attitude. But except for the tendency for those propositions holding the highest position to drop when rated for certainty as over against belief, no difference was obtained, the correlation being .99.[13] The crossing of the lines at the zero point (see Figure 1) is to be expected, since a lower position on the disbelief side of the scale would bring the rating nearer zero just as on the belief side of the scale. The results of the certainty and belief ratings do not support the statement of Sumner, that "a thing strongly believed may take a low place on the scale of certainty". It is quite possible, however, that he had a more objective measure in mind than the one used.

Another possible mode of approach appeared in the classification of the propositions initially introduced for another purpose which is discussed in Chapter V. Thirty-one junior students in Barnard College were asked to indicate in the case of each of the thirty propositions whether they constituted matters of knowl-

[11] Op. cit., pp. 618–619.
[12] Op. cit., p. 9.
[13] The rank-difference formula was used throughout the experiment in working up the coefficients of correlation.

edge or matters of opinion. The results of this twofold classification are recorded in Table IV (see Appendix)[14] which gives the frequency with which each proposition occurred under the two heads. In correlating belief and knowledge under these conditions, the ratings for belief were ranked as before, only this time without respect to sign, while in the ranking for knowledge they were arranged according to their highest frequency of classifica-

Figure 1.



Graph representing the average ratings of group I on the belief and certainty scales. The propositions have been arranged in order of highest raitng on the belief scale.

tion under knowledge by the subjects. The coefficient thus secured was .64. The correlation between knowledge and desire for the same group and under a similar method of ranking was .08; between belief and desire .76, or 12 points higher than obtained between belief and knowledge. The P.E.'s for .64 and .76 are .07 and .05 respectively. Aside from the large P.E., it is still a question as to how much significance can be attached to these results, or how conclusive they would be even if the experiment were repeated with similar results. The main objection is to the roughness of the classification upon which the ratings depend.

The scale of evidence (see Chapter I), finally resorted to, is much more satisfactory in the respect just named, since it allows for finer and more accurate distinctions. The nicer distribution of

[14] See note at end.

the ratings, as seen from Table II, seems to show that the subject was taking a more critical attitude. For a great many propositions the ratings were much lower than those on the belief and desire scales. The percentage of tens was less than half those occurring in the belief and desire ratings. The subject, in assigning a percentage value for each proposition in the two columns, was required to make these equal 100. In working up the correlation account was taken only of the ratings in the evidence column, these being divided by ten to give comparable units to those on the belief and desire scales. They might then be correlated with belief and desire when the ratings for the latter were ranked without respect to sign. The correlation obtained between belief and evidence was .42, which is still less than the one between belief and knowledge. Desire and evidence yielded practically a zero correlation, —.03.

That the evidence scale is an adequate measure of the desired relation is believed, not only because the results show a more critical attitude through the wider distribution of the ratings (see Table II), but because the subject finds himself in a more impersonal relation to the proposition's content. He feels himself more as a judge of the objective certitude of the proposition and passes upon it according to his estimate of its standing among authorities on the subject. The general tendency to assign lower ratings under these conditions may be due to the fact that "part" of what was belief is now placed under opinion. The propositions showing the greatest drop were: 1, 2, 3, 4, 11, 18, 19, 21, 22, 24, and 28. These propositions relate mainly to topics of religious or political interest.

CORRELATION COEFFICIENTS FOR BELIEF AND DESIRE

The consistency of the results appearing in Table I, and the high coefficients obtained from the number of groups represented assures us that some very definite relation exists between belief and desire.[15] Of this there can be no doubt. But do the results tell us anything about the *nature* of this relation? Can we say

[15] It might be objected that the belief-desire coefficient obtained is significant only for this particular set of propositions. As a check on this matter another group of Barnard junior girls were asked to rate for belief, desire, and evidence, the 24 propositions which Sumner selected for an entirely different purpose. From these ratings a belief-desire coefficient of .76 and a belief-evidence coefficient of .69 were obtained. The belief-desire coefficient is precisely the coefficient obtained from the first group of Barnard juniors tested with the propositions used in the present study, while the belief-evidence coefficient is somewhat higher.

TABLE I

V.F.	ρ	P.E.	V.F.	ρ	P.E.	V.F.	ρ	P.E.	V.F.	ρ	P.E.	V.F.	ρ	P.E.	V.F.	ρ	P.E.
B_1D_1	.76	.05	D_1B_5	.80	.04	B_2B_3	.83	.03	B_3B_4	.95	.01	B_4B_5	.94	.01	B_5D_5	.81	.04
B_1B_2	.94	.01	D_1D_2	.85	.03	B_2D_3	.64	.07	B_3D_4	.85	.03	B_4D_5	.83	.03	Ave.I.B_1	.76	.05
B_1D_2	.76	.05	D_1B_4	.85	.03	B_2B_4	.86	.03	B_3B_5	.97	.01	B_4B_5	.96	.01	Ave.I.D_1	.84	.03
B_1B_3	.81	.04	D_1D_4	.90	.02	B_2D_4	.56	.07	B_3D_5	.80	.04	B_4D_5	.77	.05	Ave.I.B_2	.76	.05
B_1D_3	.65	.07	D_1B_5	.82	.04	D_2B_3	.80	.04	B_3B_5	.95	.01	D_4B_5	.84	.03	Ave.I.D_2	.82	.04
B_1B_4	.85	.03	D_1D_5	.86	.03	D_2D_3	.84	.03	B_3D_5	.78	.04	D_4D_5	.89	.02	Ave.I.B_3	.86	.03
B_1D_4	.76	.05	D_2B_1	.87	.03	D_2B_4	.82	.04	D_3B_4	.85	.03	D_4B_5	.87	.03	Ave.I.D_3	.83	.03
B_1B_5	.83	.03	D_2D_1	.88	.02	D_2D_4	.82	.04	D_3D_4	.83	.03	D_4D_5	.91	.02	Ave.I.B_4	.87	.03
B_1D_5	.60	.07	B_2D_2	.78	.04	D_2B_5	.83	.03	D_3B_5	.86	.03	B_5D_5	.84	.03	Ave.I.D_4	.84	.03
B_2B_3	.86	.03	B_2B_3	.82	.04	D_2D_5	.86	.03	D_3D_5	.91	.02	B_5B_5	.97	.01	Ave.I.B_5	.87	.03
B_1D_5	.59	.07	B_2D_3	.66	.07	D_2B_5	.87	.03	D_3B_5	.87	.03	B_5D_5	.79	.04	Ave.I.D_5	.82	.04
D_1B_2	.77	.05	B_2B_4	.83	.03	D_2D_5	.81	.04	D_3D_5	.93	.01	D_5B_5	.83	.03	Ave.I.B_5	.88	.02
D_1D_2	.86	.03	B_2D_4	.70	.06	B_2D_5	.88	.02	B_3D_4	.89	.02	D_5D_5	.93	.01	Ave.I.D_5	.81	.04

Intercorrelations of the ratings on the belief and desire scales of six groups of 243 students. In the V.F. column the numerals designate the group, the letters indicate whether the factors correlated are belief or desire ratings. The last part of the table gives the average intercorrelations of each group's rating with every other.

The P.E.'s given in this table and in the correlation summary are not adequate measures of the reliability of the coefficients since they are determined by the usual P.E. formula which takes account only of the number of measures (in this case the number of propositions) and the size of the coefficient. But where the coefficient is obtained from two sets of averages the formula makes no allowance for the size of the group from whose ratings the averages are obtained. Thus the P.E. would remain constant for the correlations obtained from the ratings of several groups irrespective of their size provided the number of propositions and the coefficients were the same. So far no formula seems to be developed which would take care of this third factor, which in situations of this type should be considered in determining the reliability.

CORRELATION SUMMARY

Average of the belief-desire correlations of all groups	.81	AD	4.3
Correlation between the combined average ratings of all groups on the belief and desire scales	.88	±	.02
Average of the intercorrelations of the belief ratings of all groups	.89	AD	5.4
Average of the intercorrelations of the desire ratings of all groups	.87	AD	3.3
Correlation between the ratings of the boys on the belief scale and the ratings of the girls on the belief scale	.90	±	.02
Correlation between the ratings of the boys on the desire scale and the ratings of the girls on the desire scale	.87	±	.03
Average of the intercorrelations of the girls' ratings on the belief scale	.90	AD	5.5
Average of the intercorrelations of the girls' ratings on the desire scale	.88	AD	3.0
Correlation between the boys' ratings on the belief scale	.83	±	.03
Correlation between the boys' ratings on the desire scale	.86	±	.03
Correlation between the belief and evidence ratings of group II	.42	±	.10
Correlation between the desire and evidence ratings of group II	.03	±	.12
Correlation between the belief and knowledge ratings of group I	.64	±	.07
Correlation between the desire and knowledge ratings of group I	.99	±	.01
Correlation between the belief ratings of group III and the S.D.'s of these ratings	.92	±	.02

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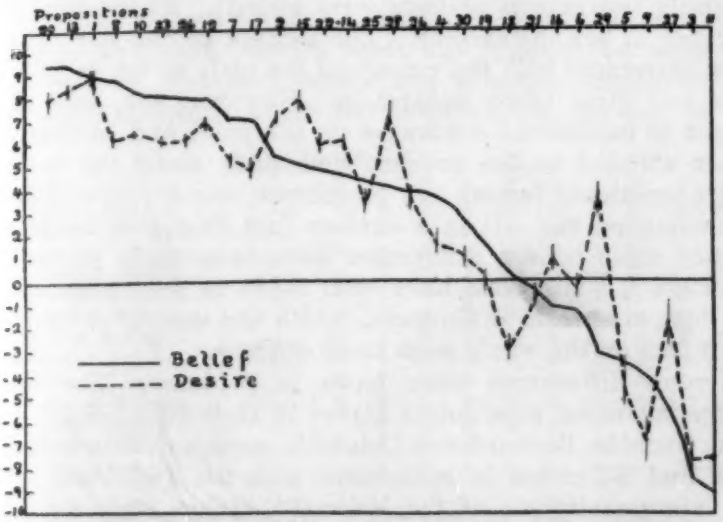
that wish and desire determine belief? Or could we with equally good reason conclude that what is believed is usually desired? In other words, is desire an important antecedent of belief or is it merely a consequent? Either conclusion might have been equally valid except for the comparatively low correlations obtained between belief and the two more objective ratings for knowledge

Figure II.



Graph representing the combined average ratings of six groups of students (total 243) on the belief and desire scales. The propositions are arranged in order of highest rating on the belief scale.

and evidence, as well as the zero correlation between the two latter and desire.

The subjects tested were members of the regular classes in Barnard College, Columbia College, and Nebraska University, and were not chosen with a view to show group differences. This might form a separate and worth while study, however.

Group I comprised 30 Barnard junior girls, group II 30 Columbia sophomore boys, group III 115 Nebraska freshman girls, group IV 20 Nebraska sophomore girls, group V 28 Nebraska undergraduate men, and group VI 20 Nebraska senior and graduate women. Groups I, II, III, and IV were members of classes in psychology.

SEX, GROUP, AND INDIVIDUAL DIFFERENCES

The correlation between the ratings of the boys and the ratings of the girls on the belief scale was .90; between the ratings of the boys and the ratings of the girls on the desire scale, .87. The average of the intercorrelations of the girls' ratings on the belief scale was .90, on the desire scale .88. The correlation between the boys' ratings on the belief scale was .83, on the desire scale, .86 (only two groups of boys were tested). These data give no evidence of sex differences. The ratings of the boys are in as close agreement with the ratings of the girls as the ratings of the boys and girls taken separately. However, the data at hand cannot be considered conclusive on the point and further studies better adapted to the problem and made along the same lines where emotional factors are prominent, might reveal differences depending on sex. It is a curious fact that practically all the studies made on sex differences have been made in the sphere of intellect, while none have been made to determine emotional and temperamental differences, which are usually acknowledged, and which on the whole seem more obvious.

Group differences were more in evidence. The Nebraska groups averaged nine points higher in their belief-desire correlations than the Barnard and Columbia groups. This is indicative of a real difference in connection with the additional fact that the intercorrelations of the Nebraska groups were considerably higher than the intercorrelations between these groups and the Barnard and Columbia groups.

Table V (see appendix) reveals the greater naïveté in the ratings of the Nebraska groups. The greatest difference in the ratings between these groups and the Barnard and Columbia groups occurs in the case of propositions 2, 4, 6, 14, 16, 19, 21, 22, and 28. These propositions relate to topics which are likely to accentuate differences in ingenuousness and moral and religious conservativism. The Nebraska groups, aside from being reared in the more religious and conservative atmosphere of their section of the West, were composed largely of grade-school teachers from country parts, and as a class more idealistically inclined. The composition of these groups was determined by the time of the year the tests were given. While the tests in Barnard and Columbia were given during the winter session of 1923–1924, the tests in Nebraska were not given till the summer of 1924. The Nebraska summer sessions are attended largely by the type of

teachers just described, coming in to take the hours necessary to the renewal of their certificates or to the securing of more advanced ones.

In view of these differences it might be expected that the Nebraska groups should be more confident of the practicability of the Golden Rule, more assured that a democracy is the best form of government, more convinced that the world came into existence through the creative act of a divine being, more ready to question the human origin of morals or even that the whale did not swallow Jonah. In the same way it might not seem surprising, that the Nebraska senior and graduate women should rank as most desirable of all the propositions, the one on the

TABLE II

Units on Rating Scales.....	0	1	2	3	4	5	6	7	8	9	10
Group I—Belief Ratings.....	6	2	3	5	5	5	8	14	11	10	31
Group II—Belief Ratings.....	5	1	3	5	6	8	6	13	11	6	36
Group III—Belief Ratings.....	3	1	2	3	4	7	7	12	9	10	43
Group IV—Belief Rating.....	2	1	1	2	4	7	10	13	10	10	40
Group V—Belief Ratings.....	3	1	2	2	4	6	4	13	11	7	47
Group VI—Belief Ratings.....	2	1	2	2	4	7	6	14	12	6	44
Group I—Desire Ratings.....	23	2	2	3	4	4	5	9	9	7	32
Group II—Desire Ratings.....	13	4	4	5	6	9	6	9	10	5	29
Group III—Desire Ratings.....	5	1	2	2	2	3	5	9	8	8	55
Group IV—Desire Ratings.....	7	3	1	2	2	4	5	8	11	8	49
Group V—Desire Ratings.....	11	3	2	3	4	4	6	5	8	5	49
Group VI—Desire Ratings.....	6	2	2	2	4	4	5	10	9	5	51
Group III—Evidence Ratings.....	14	8	9	5	4	11	5	7	8	12	17

continuation of monogamous marriage as the only socially accepted relation between the sexes, while the Columbia sophomore boys ranked it as low as eighteen in order of desirability.

The conclusion, that the differences observed are accounted for in differences in local and environmental influences, is doubtless sound. Education as such could not be the primary factor because of the high intercorrelations of the ratings of the groups in the same locality, notwithstanding the range of education represented as in the Nebraska groups.

From Table II and Table V (see appendix) it will be seen that both the individual ratings and the group averages are considerably higher for the Nebraska groups. The number of tens occurring in the ratings of the latter are increased by 50 per cent over those of the Barnard and Columbia ratings.

The frequency of unit-ratings on the belief, desire, and evidence scales reduced to persentages.

The greater frequency of the ten-ratings for all the groups concerned as over against the much lower frequency of other rat-

ings, leads one to think that people either believe or disbelieve a thing very strongly. There is pronounced evidence of this at least when individuals are asked to rate their beliefs.

Since the belief-desire coefficients were obtained through correlating the average ratings of groups, an investigation as to the nature of individual ratings and the correlations of these is of no small importance in determining the validity of the conclusions drawn from the high coefficients of the average ratings. Belief-desire correlations were, therefore, worked up between the ratings of each individual in group V and group VI. The distribution of the coefficients were as follows:

Coefficients	Frequencies Group V	Frequencies Group VI
.35 to .39	1	
.40 to .44	2	
.45 to .49	0	
.50 to .54	1	1
.55 to .59	4	3
.60 to .64	3	2
.65 to .69	2	4
.70 to .74	4	4
.75 to .79	6	2
.80 to .84	4	1
.85 to .89	1	2
.90 to .95	0	1

It will be noticed that these coefficients are about ten points lower on the average than those obtained for these groups from the average ratings. This might have been expected, however, in the same way as the coefficients obtained from the averages of all the subjects tested was higher than the average of the coefficients of the groups to which the subjects belonged. The increase in the size of the coefficients in accordance with the number of ratings represented is due to the fact that the mathematical combination of the ratings will add emphasis to any tendency which may be present. But that the individual coefficients are still largely concentrated between the significant limits of sixty and eighty adds greatly to the reliability of the coefficients of the averages.

The graded system presented in the graph (see Figure II) is the result of consolidating the ratings, and such a graph might well have been obtained though all the ratings had been tens and zeros. The difference in the positions taken by the propositions on the scale would, in this event, be determined by the amount of agreement and the balance between the number of tens and the number of zeros. Under these conditions proposi-

tions upon which the greatest divergence of opinion exists would take a zero position, since about the same number would believe it as disbelieve it. This would necessitate an increase in the variability of a proposition's rating in proportion as its position approached the zero point. This is, in fact, confirmed by group III, in the case of which the greatest number of ten-ratings occur. A high negative correlation, —.92, was obtained through correlating its belief-ratings with the S.D.'s of the same ratings.[16]

It is probably to be expected that the more naïve and less discerning a group is, the higher the individual ratings would tend to run. More discriminating individuals would find a need for more units on a scale of values and make use of the lower and intermediate units more frequently. This is in accord with Alexander Bain's theory that belief has its beginnings in primitive credulity. The primitive impulse is to affirm immediately the reality of all that is conceived. We begin as children by believing everything, and the mind of the gullible person is still in the childhood stage. On such premises simple-minded and less mature individuals would be inclined to affirm or deny things in a more wholesale manner, and in their ratings for belief-strength would use the higher units. However, our data on this point are too meagre to allow for definite conclusions.

THE PRESUPPOSITIONS OF DYNAMIC PSYCHOLOGY AS TO THE SIGNIFICANCE OF EMOTIONAL FACTORS IN BELIEF

There is much in the facts of the present study which confirm the theories entertained by dynamic psychologists. The presuppositions of these psychologists are based on a theory of instincts most clearly formulated by McDougall, who asserts, that all

[16] This negative correlation may be due to certain factors entering into the rating scheme which was used, since there is greater opportunity for variability in the case of propositions which take their positions near the zero point on the scale than in the case of propositions whose position is assigned nearer the limits of the scale. The problem then is to determine the extent to which chance factors might be responsible for such a negative correlation. In cases where chance alone operates one might expect the items or ratings to distribute themselves according to the normal surface of frequency. At the middle of the scale the distribution would assume regular proportions, but as one approaches either end the curve would become more and more skewed, since the ratings which would normally have fallen beyond the limits of the scale must now fall within these, until at the extreme limits the half of the frequencies which would have occurred beyond are now concentrated within the upper limits and overlapping the others. In this way the chance variability at the upper end of the scale would hold a ratio of 6:10 (.603 according to Ruger) to the variability at the middle. That chance is comparatively a small factor in determining the negative correlation discussed above is now apparent since the ratio between the variability of the propositions taking a high average position and those taking a low average position is 1:12.

human endeavor is determined by our instinctive nature and not by rational considerations, reason being only nature's device in securing better means toward ends suitable to nature's purposes and made desirable by innate equipment. According to this theory, it is a mistaken notion that man and animal are essentially different in that only the latter is guided by instinct while man nearly always acts on rational grounds. Quite to the contrary, however, both are under the urge of motivations not rationally determined, the only real difference being that while the animal lives the simple life of emergencies, man has given latitude to his interests and experience through his capacity to project himself into the future through imagination.

Dynamic psychology concerns itself chiefly with the emotions and instincts because it conceives of these as the basic determinants of behavior, and regards the discovery of human motivations as the central problem of psychology. In this it is not far wrong, for as a science psychology's chief aim must be the discovery of causal principles within the phenomena with which it is concerned.

With McDougall most of these psychologists conceive of the emotions as the affective correlates of instinct. Our desires, likes and dislikes, are conditioned by forces acting on this primitive level.

Upon such premises they are further led to contend that our beliefs are wish-realizations—we believe what we want to believe. We may give reasons for our beliefs but these serve only as a mask to conceal the real reasons found in emotion and desire. The latter may be tabooed and hence the appeal to reason which is more creditable. "Complexes", or systems of emotionally toned ideas, which play so large a part within the theories of the dynamic psychologist and psychoanalyst, exist in the subconscious and are responsible for many of the peculiarities of everyday experience. They may appear in the form of hobbies and disproportionately weighted interests which continually redirect our thoughts and conversation toward favorite topics. The subjective awareness of such "complexes" is by no means always present, and frequently we would vociferously deny their existence, especially if they are of a kind without social sanction. This precess of self-deception and subsequent reasoning on matters initially holding no relation to reason has become familiar in the term "rationalization". If our beliefs or conduct are questioned we feel called upon to give them the support of reason

whether they hold any relation to ratiocination or not. Such substitution of a fictitious logical process, holding little or no relation to what was fundamental to one's belief, may be made, so to speak, quite unconsciously. Such unawareness cannot be said, however, always to be present to the same extent, as in the case of the politician whose verdict on a proposition is controlled by his party affiliations. Whatever is in harmony with the platform of his party is accepted unhesitatingly, or, in the other event, rejected as quickly. Through an elaborate process of subsequent reasoning he convinces himself and others that his decision is a correct one.

But the dynamic psychologists and the Freudians in particular stress not only this more or less direct realization of "wishes" and "complexes", but emphasize the importance of "wishes" which are suppressed because they run counter to the institutions of social life and civilization. Such repressions find expression in subtle and symbolic ways, most frequently in dreams. Without adequate outlet they become the occasion for neuroses and mental disorders of a similar nature.

This brief review will suffice to show the respects in which the theories of the psychoanalytic school and the dynamic psychologists generally are coincident to, and confirmed by the high and consistent coefficients obtained between belief and desire. This does not mean that our results support nearly all of their notions nor even the major aspects of them. It only means that there is evidence that they are correct in their insistence on an emotional and instinctive basis for motivation and belief, as well as a strong bent toward idealizing the rational principle. Data bearing on the latter will be rendered more completely in the part of the study which follows.

(To be Continued)

NOTE—An appendix will appear in the more complete edition of this study which will be deposited in the Columbia University Library, after July 1, 1925.

DISCUSSION

"Modern Theories of the Unconscious" (Northridge)[1]: A Review with a Discussion of "Repression", "Catharsis", and "The Unconscious".

By W. S. TAYLOR
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I

Dr. Northridge[1] sketches at the outset interesting anticipations of the Freudian conception of the unconscious. Plato, Leibniz, Schopenhauer, Herbart, Hartmann, are discussed as important forerunners, and contrasted with the intellectualistic Descartes, Locke, the [early] British School generally, and, according to the author, Kant. Hamilton, however, J. S. Mill, and especially Carpenter, MacNish, and Abercrombie, are Britishers who mark certain advances toward the view Dr. Northridge regards as most valuable. Myers, with his theory of "the subliminal", offering important observations mixed with occultistic interpretations, is the first to be treated over a number of pages as introducing the modern group. Here Dr. Northridge, while seeming open-minded with regard to the "psychical" questions in Myers' doctrine, applies the tests of consistency and the possibility of simpler interpretation.

Considerable space is then devoted to "The Theories of the Subconscious" which have grown out of observations by the French Schools, and by James, Sidis, and Prince. Motor automatisms, multiple personalties, phenomena of religious psychology, and the various interpretations of suggestion, all indicate for the author the difficulty of explaining mental life without some theory of the subconscious. Next comes a somewhat extended and apparently tempering exposition of Freud's conception of the unconscious, repression, etc., and the vagaries of "the libido"; also the resulting conceptions of dreams, and the neuroses with their cure by psychoanalysis. The views of Jung, of Adler, and Rivers, finally, are sketched, not without some penetrating criticisms, Occam's razor being wielded with especial effect upon Jung's conception of "the collective unconscious". Jung's less stereotyped methods of dream interpretation, however, Dr. Northridge regards as better than the Freudian leaning upon orthodox "symbols".

Although the whole trend of the volume is such as to support many of the fundamental Freudian conceptions of the unconscious, as already suggested Dr. Northridge's devotion is by no means slavish. Thus

[1] *Modern Theories of the Unconscious.* By W. L. Northridge, M.A., Ph.D.; with an Introduction by Prof. J. Laird, M.A. The Edinburgh Press. E. P. Dutton and Co., New York, 1924. Pp. xv + 194. Price $3.75.

he regards a number of the more extreme types of Freudian interpretation as far-fetched; he would replace Freud's conception of "the libido" by McDougall's "simple enumeration" of instincts; he finds that "the failure to distinguish clearly between affect and conation cannot but be puzzling to the ordinary reader of psychoanalysis"; also, that in the Freudian use of the term "repression", "there seems to be some confusion"—confusion between "repression into the unconscious" and "that type of forgetting in which the contents of consciousness are simply thrust into the fore-conscious" (a process Northridge would call "*sup*pression"); and he suggests that the notion of "unconscious mental elements" involves a contradiction (unconscious consciousness) which can perhaps best be resolved, not by the worse contradiction of regarding the results of *experience* as *brain* modifications, but by assuming that "the unconscious consists of *mental* dispositions or traces or tendencies" (pp. 184–5, italics the reviewer's),—a metaphysical position in which he consciously accords with such psychologists as Ward and Stout, as well as with some of the suggestions made by Bertrand Russell.

But the author agrees with the Freudian psychoanalysts that we must include in the unconscious proper, as contrasted with the fore-conscious or preconscious, "those dispositions or tendencies that are not capable of finding their way into consciousness except in disguised forms, until aided by artificial methods". The preconscious, on the other hand, properly should stand "for the material of the unconscious that can be recalled by the act of attention" (186).

II

A critical historical survey of theories in a particular field can be very useful, even though such a sketch is confessedly limited in scope or point of view, provided there is fair perspective within the limits set. And that Dr. Northridge's attitude is essentially that of the kindly searcher for truth, is evident. It is regrettable, therefore, that he has not appreciated more of the modern conceptions.[2]

This lack seems to be due partly to the *presuppositions* Dr. Northridge, like so many others, has developed with regard to his subject-matter. Thus it is noticeable in his sketch of the work of Janet and of Prince, that the author finds evidence there for the existence of *the* subconscious,—a something, an *entity*, apparently, instead of a more or less active mass of more or less closely and even variously integrated processes. Thus of course, along with those who lean upon the philosophy of "the unity of the organism" instead of upon the psychology of the psychopathic organism, Dr. Northridge is shut out effectively from an understanding of some most

[2] The deficiencies in his interesting sketch of earlier views are less important, though noticeable: Spinoza is not mentioned at all; neither is Hume's great appreciation of the rôle played by instinctive feelings in our life (*Treatise* ii 3, 9); and there is no reference to those important observations of Kant which Dr. Prince has quoted from in *The Unconscious*, p. 251n.

illuminating psychopathic phenomena. And perhaps the same presupposition is what accounts for his loosely lumping (in this connection) Janet, Sidis, Prince, and the New Nancy School, saying (p. 73) "they speak of a subconscious self and subconscious personality." Yet in the same context he states that "these writers avoid such expressions as 'subconscious ideas'." (Cf., to the contrary, Janet's examples and explanations in *The Mental State of Hystericals*, 1901, pp. 284–285 [originally reported in 1889], 325, and 410–412; and Prince, *The Unconscious*, 249–254, also *The Dissociation* in many places; also Northridge's own exposition of Prince on p. 92.) It is not surprising, therefore, to find the author subscribing to the widespread belief that Janet, and Prince, respectively, describe everything in terms of dissociation simply, without bringing out any principles to explain the dissociation itself. Dr. Northridge's criticisms of Sidis are perhaps more adequate.

It is especially regrettable that in this presentation of modern theories of the unconscious we find no recognition of that fundamental principle, at once the real basis of all psychotherapies, and a great but too-little-appreciated common bond between abnormal psychology and laboratory psychology: the principle, namely, of reëducation. Thus Dr. Northridge, in his examination of Janet, misses altogether the force of that investigator's observation that it is the *modification* of a dissociated memory that effects the cure of the patient on that point (Janet, *loc. cit.*), the various modes of "analysis" being only the means of discovering *what* the memories are that need to be so "re-set," as Prince has described the situation. In the same way, although the author himself distinguishes neurotic cases of repressed *meanings* (p. 187), and cites the theory of "neurograms", he nevertheless gives no account of Prince's *experimenta crucis*, reported in *The Unconscious*, which demonstrate the fundamental rôle of "settings", or naturally acquired meanings, in the etiology of the neuroses, in illuminating the nature of "the subconscious", and in the understanding of everyday behavior (*op. cit.*, pp. 321–422).

True, the author does recognize some general points of affinity between the methods of Prince and those of Freud. And he reports the distinctions Prince observes between unconscious and co-conscious and subconscious. But it is clear that in regard to these concepts Dr. Northridge is (at least at times) badly confused (pp. 73, 92). Also, he supposes (pp. 89–90) that Prince thinks that all impressions that have ever befallen the individual are conserved and awaiting recall.

Thus a more thorough knowledge of modern researches would have enabled Dr. Northridge to place his critical exposition of the Freudian theory in a perspective which would have been most valuable. As it is, however, in spite of occasional close mixture of the author's own ideas with those he takes to represent his sources, the volume is more understanding, cautious, and adequate, than many current works.

III

When a new theory, like the Freudian, is not very sensitive to values in the work of others in the same general field, the complete soundness of such a theory is naturally suspect. And when the theory is expressed in a misleading language which savors of the primitive and metaphysical rather than of the descriptive, we should disclaim finality for its formulations. And, finally, when the theory on the one hand has to leave out of account many important complexities, and on the other, embraces a number of inconsistencies and unwarranted statements and inferences, it is obvious that thorough reorganization is necessary. Such studies as Dr. Northridge's serve to emphasize this fact.

To illustrate specifically:

Perhaps the most misleading of the Freudian formulations are those especial concerns of "the unconscious" called "repression" and "catharsis". The way in which these notions mislead, though supposedly stated on the basis of clinical proof, may be indicated by the following query which came to the present writer from an economist friend: "I would like to ask", he says, "what the psychologists would have us do to cure our prejudices. I have heard some say that the correct thing to do is to trot them out and think about them, and perhaps exercise them consciously. That seems to me to be a dangerous proceeding, as likely to start a habit. Of course, merely recognizing a prejudice will not start a habit, but what good would mere recognition of a conflict or prejudice do in solving the conflict or removing the cause of the prejudice?"

Thus this man hits the nail on the head. If only the enthusiastic advocates of the creed of "effecting emotional catharsis of dammed up libido", and of "bringing submerged memories to the light of consciousness",[3] would allow for this habit-formation principle of both popular and scientific psychology, they would not be so liable to turn out for libidinous delectation such Utopian(?) pronouncements as, *e.g.*, Maurice Parmelee's *Personality and Conduct*, or sundry "psychoanalytic writers'" illuminations on "movie" censorship, art, morality, etc. That these examples are extreme, and contrary to some of the express statements of the founders of psychoanalysis, must be admitted with emphasis; but these cheerful "followers" of Freud may be pardoned for their excessive inferences, since Aristotle agrees with Freud perfectly on the matter of "catharsis". What is overlooked, however, is the fact that it was Aristotle, the student of esthetic, whose view agrees with that of Freud; Aristotle as moralist preaches a different and convincing doctrine. (That there is *something*, and something important, in the notion of "catharsis" as such, appears to be true; but that is another matter.)

Of course if emotion were a Stuff, which if not drained off impulsively

[3] This last policy, "Translating the Unconscious into the Conscious," is Freud's later formulation. Ff., Freud, *A General Introduction to Psychoanalysis;* pp. 375–376. New York: Boni & Liveright, 1920.

is in danger of so working through "the unconscious" as to even get "converted" into some bodily symptom, then the ideal of a most unrepressed society would be the one for which we all ought to work as hard as possible. But apparently there is adequate experimental evidence to prove that emotion is not a stuff for which the only requirement is "catharsis". (I am not speaking now either of vesicular or equivalent pressure, or of unused adrenin, *e.g.*, and their possibilities for nervous irritation. These are real questions, and of real importance to psychology and ethics.) *Mere* "emotional catharsis" never cures any prejudice or neurosis, as the Freudians early observed. Also, however, *mere* "bringing to the light of day" never straightens out any trouble; as Dr. Prince has shown in *The Unconscious*. The cures that do result when these practices are professedly engaged in, result from the fortunate incidental fact that in the process, in the confusion of burning down the house to roast the pig, the beast's organization gets changed: in recovering the "submerged affects" of the patient, interesting him in the problems of his dreams, etc., he is led to, or hits upon, new linkages with, new attitudes toward, his old memories; he gains new associations, re-conditioned reflexes, re-education.

For example: A woman of about forty years of age is seized with fear and anguish whenever she sees a church steeple or tower of any kind. Dr. Prince by means of hypnosis recovers for her some girlhood memories of the time when her mother died. It then develops that the patient was full of self-accusation for her own carelessness which she thought brought on her mother's illness. During that illness and subsequent operation the girl prayed and cried constantly. At the same time the church bells were ringing, getting on her nerves so that she hated the sound, and came to fear any ringing of bells (the real object of her phobia). Upon remembering these facts, under hypnosis, she sheds many tears ("catharsis"); but is not cured. Then she remembers the same things after waking from hypnosis; still no cure at all. But *then* Dr. Prince enables her to remember other facts, which prove that it was not her carelessness which brought on her mother's death; the patient suddenly acquires a new point of view with regard to the whole matter; and she is cured. It was necessary to convince the patient that she was not morally responsible for her mother's death before a cure could be brought about. (*The Unconscious*, pp. 389–410.)

The same principle is beautifully demonstrated by recalling the shocking memories to the mind of a patient who is under hypnosis, and then recalling the reinterpreting memories and establishing a sounder point of view, giving a new meaning to the buried experiences, all while the patient is hypnotized, and without having those memories brought into the waking consciousness at all. The cure results just the same.[4]

[4] The relative values-in-practise of these two methods (resetting what become waking memories, and resetting memories which are allowed to remain subconscious), is a different problem. Questions like keeping the patient's confidence, etc., would seem to enter here.

Again, the same principle appears to explain those cases in which a patient is made worse by psychoanalytic efforts. In such instances, the Freudians speak of "incomplete analysis"; or as Dr. Hadfield puts it: "The most real danger in amateur analysis is that it may raise questions and arouse emotional forces which it cannot control. It is essential that, when an analysis is commenced, it be carried through to the end. If we simply probe into a shrapnel wound without extracting all the shrapnel, we make the patient worse: when it is completely extracted the pain ceases and the wound heals. So it is with the mind." (*Psychology and Morals,* 131n.) But would it not seem most reasonable to regard those exacerbations as the natural result of *practising* the emotional tones of bad memories in the process of recognizing them?

For in this connection it should be acknowledged that to encourage emotional expression during analysis seems to help the recall of memories, if only because such emotion is associated with the memories in the mind of the patient; the recall of memories being effected, as already remarked, only to find out on what points the patient needs re-education.

This is indeed to admit that there are many elements of truth in the *procedure,* though not necessarily in the supposedly corresponding *theory* of the orthodox psychoanalysts. But restatement should free those elements of truth from the utterly misleading implications of the current doctrines.

Much more could be said, and needs to be said, with respect to the urgency of differentiating between those several, importantly distinct, processes roughly comprehended by the term "repression". But perhaps enough has been done with this topic in connection with the notion of "catharsis" to illustrate the need for general reformulation of the Freudian ideas.

The question which immediately occurs is, if these supposed implicates of "the unconscious" are so capable of critical reduction, what of "the unconscious" itself?

Dr. Northridge is evidently interested in this problem, for he raises the question as to whether those processes beyond the "fringe" are conscious or not. But to the present writer this seems to be a question which is, as yet at least, one of the *innocuously* theoretical sort. That is, it appears that the question is incapable of any logical or experimental proof either way: it is purely a matter of the metaphysical and epistemological assumptions one brings to bear upon the problem. Also, no difference would result for practice, apparently. The subconscious processes may be unconscious or co-conscious, or even "rapid oscillations of different dissociated systems of consciousness, each for the moment becoming the personal consciousness, but with amnesia for the preceding moment", for all we really know, as suggested in a recent editorial in this JOURNAL.[8]

[8] "The Actuality and Nature of Subconscious Processes," JOURNAL OF ABNORMAL PSYCHOLOGY AND SOCIAL PSYCHOLOGY, 19:130 (July–Sept., 1924).

But we do know, through abundant demonstration, that subconscious processes, as processes, are often of a highly complex character, and occur in varying degrees of independence of that most complex system of processes that we call "the main personality".

Whether this requires the employment of "physical" or "psychical" conceptions exclusively, again, is perhaps a most important problem; or perhaps it is only a question of descriptive terminology. Possibly, indeed, it is not really out of order for us to employ both sorts of language interchangeably: possibly it is only some unexamined allegiance to Cartesian conceptions which would seem to require "ontological consistency" for our nomenclature. But however that may be, whatever the color of the metaphysical spectacles one sees through, or whatever the ontological stuff in which the problem arises, the structure or type of organization of "the unconscious", and the principles which describe the operations of subconscious processes, are worthy of presuppositionless observation.

And what are those principles, and the type of organization? Here opinions differ. But to many students, agreeing in a number of points with Dr. Northridge, the evidence is clearly against the assumption of (to sketch roughly in composite) (1) an entity, (2) which mechanically echoes its supposed past (archaic symbols, etc.), and (3) speaks a cut-and-dried language now (orthodox dream symbols, etc), (4) wishfully antithetical to civilized life, or else (5) mystically overflowing with ambition and spirituality, and (6) peculiarly close to a living soul of the race; all (7) compressed somewhat effectively by a well-meaning censor or two. The evidence rather is in favor of, as was said above, a mass of hereditary and acquired processes which may be organized in all sorts of ways; processes which have indeed a history, but a history which requires scientific tracing in each individual case; some of whose associations may appear variously as symbols; whose elements, in so far as they become to any serious degree distinct from the individual's daily scope of attention, are become so distinct in many cases because of a conflict with the waking elements, hence may well comprise rebellious elements, but elements normally capable of integration into any scheme of real civilization; an integration in which conflict,—which is truly of many sorts and complications, including the types called "censorship"—instead of being followed by evil "repression", resolves into normal inhibition of those impulses that threaten the organism's major interests.

This means a concrete and relativistic, rather than an abstract and absolutistic, theory of the "the unconscious". True, the latter is easier to grasp, with the imagination at least, and so gives quicker comfort to the unquestioning in times of perplexity. But as statistical and experimental tests multiply, that theory must win which is explicatively applicable to more cases and more phenomena.

CORRESPONDENCE

The Powell Lectures on Psychological Theory at Clark

Editor Journal of Abnormal and Social Psychology:

It may interest your readers to learn of a plan which has been successfully inaugurated at Clark University to bring before the graduate students of psychology and others interested the divergent theories of the different psychological "schools".

It has long been the belief of the writer that most graduate students in psychology do not get a fair presentation of the various significant "schools" in psychological theory. The leaders in psychological theory are naturally biased towards their own methods of investigation and interpretation and their students develop the same points of view. It is practically impossible to imagine that one of Titchener's students will ever develop into a "behaviorist", and equally difficult to visualize one of Watson's students becoming a "structuralist". It would seem to be highly desirable to have graduate students receive at firsthand the theories of method and interpretation held by the leaders of such groups.

Dr. E. E. Powell, a professor-emeritus of philosophy, has thought so well of this project that he has generously financed the lectures. Each lecturer delivers either two or three lectures before the members of the Department of Psychology, but they are open to the public which has attended in large numbers.

On the evenings of December 12, 15 and 17, 1924, Dr. Morton Prince, representing the "purposive" group in psychology, lectured on the subjects: "Three Fundamental Errors of the Behaviorists and the Reconciliation of the Purposive and Mechanistic Concepts"; "Awareness, Consciousness, Co-consciousness, and Animal Intelligence from the Point of View of the Data of Abnormal Psychology—a Biological Theory of Consciousness"; "The Problem of Human Personality—How Many Selves Have We?"

On January 16 and 17, 1925, Doctor John Watson, representing the "behavioristic" group, gave three lectures on, "What the Nursery Has to Say About Instincts"; "Experimental Studies on the Growth of the Emotions"; and "Recent Experiments on How We Lose and Change Our Emotional Equipment."

On the evenings of April 20, 21 and 22, 1925, Doctor Knight Dunlap, representing the more reconciling group in American psychology, will lecture on topics to be announced later.

Other leading exponents of significant methods in psychology will be invited to lecture and dates will be announced later.

The various lectures as delivered will be published in *The Pedagogical Seminary* and *Journal of Genetic Psychology*, after which they will appear in a single volume.

Carl Murchison.

Clark University.

A Memory of Infantile Life—A Scrap of Personal Experience

Editor, Journal of Abnormal and Social Psychology:

On page 394 of the Second Edition of "The Dissociation of a Personality" [by the editor] you say: "A memory going back to infancy is without doubt an hallucination. . . ." I have found similar statements in the works of Bernard Perez and others. My own recollection makes it difficult for me to accept such statements when not qualified by a consideration of the stimuli responsible for the emotions and resultant actions which people claim to remember. I am therefore led to send you the following personal experience:

I was born December 23, 1872, in a sparsely settled section of West Virginia. Our nearest neighbors were probably a half-mile distant.

In that community, in those days, neighbors went visiting, uninvited, and stayed all day—a very informal custom.

On such an occasion there came to our home a woman with a baby which wore a long, white dress. As I visualize it now, the dress was nearly, or quite, twice as long as the baby.

My mother called the woman (Mrs.) Grossman. I do not actually remember the "Mrs.", but I distinctly remember the name, "Grossman", and I knew, at the time, that it was connected with the woman. I do not remember any mention of the baby's name at the time.

The women must have temporarily exchanged babies, for I found myself on the strange woman's lap, and I saw the other baby on my mother's lap. What my emotion was, jealousy or fright, I cannot recall, but I became fretful, and the woman (no doubt thinking I was hungry) offered me the consolation of her breast, actually putting the nipple to my lips. I was so repulsed that I rebelled—I squalled, whereupon the mothers hurriedly exchanged babies, talking the while in soothing tones. The words were unintelligible to me at that age, and I remember but one of them, *viz.*, "bony". But even to this day, I can plainly see, in my mind's eye, the excessively developed and drooping mammary organ, and I clearly recall my stubborn resistance to her kindly offer. I know I wriggled vigorously on her lap, but I have no further recollection of the incident. The woman's face is a blank, and her dress has faded from my vision. But when, at the age of five, I went to the district school, and heard a boy called Cavalla, the name was instantly connected in my mind with the incident on the occasion of Mrs. Grossman's visit. Of course, I did not recognize the boy's features, but I had not the slightest doubt as to his identity, and in that I was right, for that boy proved to be Cavalla Grossman, and I have never known any other person either christened or dubbed "Cavalla".

I saw Cavalla Grossman often that winter at school, though for reasons below made obvious I never, to my knowledge, saw his mother on any occasion except the one mentioned. I had a feeling of aversion for the boy, and though we moved away from that community when I was about six

years old, the unpleasant memory remained with me. I have seen him probably twenty times since we grew up, and on every occasion the thought of that one incident was uppermost in my mind, and remained so as long as we were together. He grew up to be a successful business man, and there is no justifiable reason why he should be repulsive to me.

Owing to a boy's natural diffidence, I never mentioned the incident to any one until I was probably twenty-five years of age, when I asked my mother about it, and the conversation at that time ran, in part, as follows:

"Mother, was Mrs. John Grossman ever in our home?"

"Yes, son, but you do not remember; you were only a baby."

"Very well, mother, was anyone with her?"

"Yes, her baby, Cavalla; he is just two weeks older than you."

"How old was I, mother? Be careful; I do remember."

"Nonsense! You were an infant in arms."

"Was Mrs. Grossman ever there at any other time?"

"No, son, I did not visit Mrs. Grossman."

I then related to my mother the incident in question, and tried to help her remember it, but she could not. When asked if she had any idea what she meant by the word "bony", she replied that being Scotch, she probably used the word "bonny". However, I do not think she did.

My mother somewhat reluctantly admitted that the incident might have occurred, as neither child had been weaned, and that owing to the isolation of our home such visitors were few. When questioned as to why she so distinctly remembered Mrs. Grossman's visit, she answered simply, "I explained that a moment ago. It had nothing to do with your imaginary troubles." From her answer I concluded she was referring to her former statement that she did not visit Mrs. Grossman, and also that she was not a very good witness for me. Nevertheless, she did not shake my confidence in my memory.

I have never had any nervous trouble of any kind, and feel sure that I have always been correctly considered quite normal physically. My memory has always been considered among students and scholars as exceptionally good in matters of history, literature, languages, current events, etc., but it is by no means prodigious, and, in fact, I remember nothing further until I was two years and eight or ten days old, when, upon the birth of a sister, January 1, 1875, my anxiety was aroused by the teasing remark of some older person that my new sister was no good because she had no hair. I went to the bed to investigate. Peculiarly enough, I remember the intervening space, the character of the floor over which I passed, the bed, and my mother in it, but I cannot remember the object of my quest. I have no mental image of that sister until when she was learning to walk she fell and cut her forehead.

I am giving my experience for publication, hoping that others may be encouraged to come forth and help to clear up the vexed question of infant memory.

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January 3, 1925.

REVIEWS

Social Psychology. By Floyd Henry Allport. Boston. Houghton Mifflin Company, 1924. Pp. xiv + 453. Price $2.50.

Without doubt, this book represents a definite forward step in the (incipient) science of social psychology. It has a textbook quality indicating that the subject has now been hammered into such shape as can be presented in a rather systematic and formal way. Some previous books, fully alive to the social problems requiring psychological interpretation, have made little use of actual psychological knowledge; others, while definitely psychological, have not dealt in an elementary way with the group situation. The present text is really comprehensive. It is systematic and well-knit. And it is thoroughly serious, not intended simply to stir up thought in the student (though it certainly will do that), but to place real knowledge in his hands and lead him to, or towards, the true solution of the problems attacked. In short, it has more the air of a scientific textbook than any of its predecessors.

In token of its modernness, the book lays claim to adopting the standpoint of behaviorism, as a convenient system for conceiving and explaining human actions. The author is not, indeed, a behaviorist in the strict sense, since he makes use of such concepts as "social consciousness," "imagery", "awareness", "impression of universality", and is concerned with such problems as the perceptual aspect of instinct and the James-Lange theory of the emotions—all of which the strict behaviorists seek to wipe from the psychological map. Exactly what the author means by adopting the behavioristic standpoint is not clear. He likes to state his facts and theories in objective terms wherever possible. He regards the physiological as the only explanatory statement of the facts, but this view, of course, is not peculiar to behaviorism. Also, he reaches what he calls an "austere" view of human life, which might be more aptly called a "hard-boiled" view—a type of view that is rather characteristic of behaviorism and perhaps constitutes one of its main attractions.

A second token of modernness is the considerable use made of Freud. The author is not exactly a Freudian, for he has little to say of the unconscious, he does not base all human motives on sex, nor on sex plus the "ego", nor does he accept the dogma of infantile sexuality; but he has evidently found the Freudian approach very suggestive and illuminating in the study of personality.

A third sign of the times is the first textbook appearance of experiment in social psychology. The experimental evidence introduced is necessarily meager as yet, and not always conclusive. For all that, this first attempt to base the psychology of groups on experiment is of immense importance as a forecast of the future of the science.

The author's treatment of social psychology runs the gamut from physiology to sociology. At the physiological end of the scale, we find accounts of reflex action and of the autonomic system, and considerable detail regarding the muscular machinery of speech and of facial expression. At the sociological end we find judicious observations on democracy, city and country life, religion, government, industry and commercial exploitation. Between these extremes we find the properly psychological core of the work, dealing with the innate basis of behavior, the modification of reflex behavior by learning (which is half "conditioning", and half trial and error), the analysis and measurement of personality, response to social stimulation in the crowd, the co-acting group and the face-to-face group, social attitudes and social consciousness, social conflicts and their adjustment. Especially valuable, as contributions to the progress of the science, are (1) the careful working out of the part played by conditioning and trial and error in types of behavior which, as viewed in the adult, seem to be fundamental and are often heedlessly labeled "instincts"; (2) the analysis, partly experimental, of crowd and group activities; and (3) the discussion of conflicts arising in family life.

This book is not only useful as a compendium; it is of no little importance on the theoretical side. It might be called decidedly ambitious on this side, for the author has his own theory on almost every question, and each of his theories is carefully worked out and worthy of serious consideration.

The chief importance of the work, on the side of theory, lies in his resolute attempt to prune away unnecessary assumptions, and build up a social psychology on the fewest possible fundamentals. He prunes away altogether the social instincts, such as self-assertion and submission, pugnacity, gregariouness, and the sex and parental instincts; in fact, he endeavors to prune away all instincts of any degree of complexity, and leave as native fundamentals only the simple reflexes, plus the power of learning. To accomplish this reduction of the accepted native basis of all behavior is one of his two main objectives. The other is perhaps no less ambitious, though on less familiar ground to the psychologist. He wishes to do away, not only with such concepts as group consciousness, group mind and mob mind, but even with group behavior. All behavior consists of the responses of the individual to stimulation. Other individuals, and groups of such, act as stimuli, but the response is always the response of the individual. There is social stimulation, but no social response or behavior. Though these statements seem innocent and obvious at first, I shall try a little later to show that they involve a serious omission from the fundamentals of social psychology, and that this omission leads the author to overlook the most important part of his subject.

However that may be, there can be no doubt that the author's deliberate and thoughtful effort to get down to the fewest possible fundamentals is of great importance in the history of the subject. We never know whether we need a thing till we try to do without it. Psychology could

not know whether it had need of the concepts of instinct and group behavior till a psychology was carefully worked out without making use of them. Now that the author has performed this service for us, the next step is to examine the result for its adequacy. In other words, duty now demands that we attack the author tooth and nail, so as to discover whether he has left himself resources enough for a successful defense.

The first main question at issue is whether the author builds up an adequate psychology without the concept of instinct. In rejecting instinct, he does not intend in the least to deny the existence of native reactions, but only to reduce them to very simple terms. He does not belong to the school who would have the individual a *tabula rasa,* in respect to behavior. He stands quite with McDougall in regarding all human behavior as derived, in the development of the individual, from a stock of native reactions. Only, he would recognize as native reactions only reflexes of very simple type.

The concept of instinct he regards as "dangerous", apparently because, in unskilled hands, it is used to cover adult activities that have been built up by the process of learning on the basis of some rudimentary reflex. This danger has of course been repeatedly pointed out by psychologists, as by McDougall and by Thorndike. But to the author the danger appears so serious as to warrant the total exclusion of the term, "instinct". He must be thinking of readers so socially minded, so little physiologically minded, that, on hearing of a sex instinct, they incontinently conclude that human marriage and family life are instinctive, or, on hearing of a gregarous instinct, that patriotism is instinctive. For such readers, instinct is certainly a dangerous term to use in a book on social psychology.

But this is not the whole danger. The convenient term, "instinct", according to the author, leads even psychologists to overlook learning and the influence of the environment in the development of regular forms of behavior. In view of the fact that some of the most prominent instinct-psychologists have at the same time been ardent students of the learning process, this danger can easily be overestimated. Our author himself does nothing essentially different from his predecessors in tracing the modification of native reactions. Like McDougall, even, he shows how a native reaction becomes modified, on the afferent side, by the substitution of new stimuli for the original or native stimuli, and, on the efferent side, through trial and error, by the selection of the best-adapted of the native variants of response, and by combination of native responses into more complex, integrated performances. He does, to be sure, make freer use of the concept of "conditioning" than any of his predecessors, and makes very telling use of it; but there is nothing essentially new in this concept, except for the still meager facts of the conditioned reflex proper, and except for the dubious assimilation of other instances of "conditioning" to the conditioned reflex as the type.

But what the author chiefly desires to accomplish by his elimination of instinct is, after all, rather radical. He desires to limit the native factor

in behavior to very simple reflexes. All coördination and integration, beyond the stage of simple reflexes, he ascribes to learning. All ripening of instinct, after birth—all specific maturation within the nervous system—is excluded. Native reflexes are "crude", poor adaptations, and that in two ways. They are crude on the afferent side in not being specifically responsive to complexity in the stimulus; their native stimuli are exclusively simple environmental agents, such as heat and cold, loud noises, loss of support, painful and tickling stimuli. On the efferent side they are crude in being simple, random, lacking in precision and integration, and only very roughly adapted to the needs of the individual. It is only through "conditioning" that they become attached to complex, meaningful stimuli, such as useful or dangerous objects; and it is only through trial and error that they become precise and adaptive and integrated.

Human walking, for example, is not an instinctive performance. Rather, it is built up out of a large number of simple reflexes, such as kicking, stiffening the knees, and holding the head upright. Each of these separate reflexes, at first occurring with a motley assortment of other reflex movements, is selected and perfected by practice; and finally, when the peripheral organs are sufficiently developed and when the *general* plasticity of the brain has matured to a sufficient degree, the necessary cerebral connections are formed, and the integrated movement of walking appears.

In this specific instance of walking, the author's conclusion is difficult to accept in the face of a certain amount of evidence which he quietly ignores. Cases of "birth paralysis", affecting the lower limbs, are instructive in this connection. A child so affected may show no deficiency whatever in general brain plasticity or power of learning and adaptation. His muscular and sensorial development remains good. Moreover, he may finally learn to walk, thus giving evidence that all the absolutely essential neural connections in the lower centers are present. Yes, he finally learns to walk, perhaps by the age of eight or ten, after years and years of carefully supervised practice. This genuine learning to walk is very different from the relatively easy process which the normal child goes through in a few weeks or days. If nature provided only crude random action of the various motor organs, and left all selection and integration to individual learning, we may well doubt whether human locomotion would be as universal and uniform as it is.

In view of the general acceptance of maturation, of the considerable mass of prima facie evidence for it and the practical certainty of its occurrence in some animals, pretty convincing contrary evidence is needed to rule it out of court. The author offers three main lines of evidence. First, the neural mechanism required for a complex integrated performance such as walking is itself so complex that it cannot conceivably be determined in the germ plasm. Well, the structure of the heart is complex and well integrated, and yet is determined by heredity and not by

individual trial and error. The *a priori* argument from inconceivability is not as convincing as it used to be.

In the second place, it is stated that the internal development of the brain, after birth, shows no sign of specific lines of connection, but only of general growth. How such a statement can be ventured is a mystery, in the face of myelinization tract by tract. Certainly there is structural maturation of a very specific sort after birth. And there is good recent evidence (Tilney) that specific functions, such as opening the eyes or walking, in puppies, make their appearance with the myelinization of specific structures.

The author's third line of evidence against maturation consists in a reinterpretation of Shepard and Breed's well-known experiment on the pecking of young chicks. A chick, given a chance on the second day after hatching to peck grains of wheat, actually secures and swallows 15 per cent of the grains pecked at; he improves rapidly from day to day of exercise till he reaches a maximum of 85 per cent; another chick, prevented from pecking till the fourth, fifth or sixth day after hatching, makes no higher average at first, but improves more rapidly to about the same maximum. Our author's interpretation of these results is as follows:

"On the one hand, the results of this experiment discredit, for the activity concerned, the theory of the specific maturation of an instinct, and show the necessity of practice as in all habit formation. The low beginning and improvement of the delayed groups show that in their case, as in the case of the undelayed, the response of pecking had to be *learned*. On the other hand, the capacity of the more mature chicks for more rapid learning indicates that they were able to profit in forming the pecking habit by a greater *general development* of neurones, synapses, receptors, and effectors than existed in the birds one day of age."

The results are indeed consistent with this interpretation; but they do not force us to it. All they really show is (1) the necessity of practice, and (2) maturation, either specific or general, but whether specific or general they do not show. In other words, they are equally consistent with an alternative interpretation, to wit: the inherited mechanism for any given response goes through a specific maturing process, part of which may occur after birth (or hatching); but such maturation always leaves the mechanism in a condition capable of perfectionment through exercise. This formula is certainly correct as applied to a muscle, and I submit that it is extremely probable as applied to a neural mechanism. At any rate, since the experimental results are fully as well explained in terms of specific maturation plus practice as in terms of general maturation plus practice, they afford no support to the author's brief against specific maturation. In short, his brief contains no positive evidence whatever in favor of his contention, but leaves the question entirely open; while it attempts no rebuttal of the considerable mass of evidence on the other side.

Attention should also be called to an obvious misinterpretation contained in the above quotation. We read there that "the response of

pecking had to be *learned*". Not so! The movement of pecking, just as a movement—and a complex coördinated movement at that, requiring harmonious action of legs, trunk, neck and bill—was executed from the start. The chick pecked from the start; the only question was whether the direction of the peck was accurately controlled by the visual stimulus, and whether the bill movement was timed just right to seize the grain. Quite good precision was necessary in order to score in this test. It is clear from the results, not only that the pecking movement, as a movement, did not have to be learned, but even that its control by the visual stimulus did not have to be altogether learned; otherwise the score of perfect responses would not have been 15 per cent on the first day, but close to zero. If the chick, on receiving the visual stimulus from a grain of wheat, had simply reacted "at random", then he might not have pecked at all; he might have scratched; but if he had pecked, he would have pecked at random, in any direction, and, in the fifty trials that he was allowed on the first day, he would probably have secured no grains at all, instead of seven or eight. If he had to build up his whole "pecking habit" out of really "random responses" by trial and error, it is safe to say that his progress would be very much slower than the results show.

In general, the author much overrates the "randomness" and "crudity" of native responses. Whether a baby's actions shall be called "crude" or not depends upon the point of view. If we look at the baby as raw material to be fashioned according to the specifications of the social adult, then the baby is crude. But if we look at him just as a baby, then his bawling, kicking and squirming—not to speak of his breathing, swallowing, sneezing, etc.—are pretty good adaptations to the situation of a baby. The concept of "crude native reflexes" is a "dangerous" one; it is unfair to the baby, both scientifically and practically.

The concept of "random responses" is also dangerous. Many of us, including the present reviewer, have been guilty of a loose employment of this phrase. But I am convinced that few if any really random responses are ever observed. Any reflex is a highly coördinated performance, on the motor side, and also shows some relation to the stimulus. Our author himself says as much, in his opening remarks on behavior: "The response usually has some characteristic relation to the stimulus." This excludes the notion of "random response". The concept of "randomness" is dangerous, because it leads us to imagine that, in bringing up a child, we have to do with an utterly formless creature, to be wholly molded by social and other environmental influences.

The concept of "habit" is similarly dangerous. The author speaks of the "pecking habit" of the chick. Shall we conclude that pecking is "just a habit" with the chick? Such an expression would imply that no inherent propensity of the chick underlay this reaction, so that the reaction could be dropped without any alteration in the nature of the chick. "Instinct" carries the implication of native propensity, and habit that of regularization and automaticity. The one is a necessary complement to

the other, in the description of behavior. If "instinct" is thrown overboard as dangerous, then, by all means, let "habit" go with it, to preserve the balance.

Our author, however, preserves the balance in another way. He introduces a new term to carry the implication of "propensity" without any implication of complexity of native response. His new term, "prepotent reflex", is indeed borrowed from Sherrington, but given a new meaning. Sherrington spoke, quite simply and logically, of one reflex as being prepotent over another. The protective flexion reflex of a hind limb is prepotent over the stepping reflex of that limb, and this in turn over the postural reflex of the same limb. It is a question of one reflex giving way to another when the stimuli for both are present. The term, in Sherrington's hands, points to a relationship between different reflexes. Allport uses the term in an absolute sense, or nearly so. A certain number of reflexes are classed as the prepotent reflexes, not from any study of their relation to other reflexes, but, apparently, because they represent the most distinctive propensities of the individual. We read also of prepotent needs, demands and requirements; and there is a long list of "prepotent habits", which, being rooted in the prepotent reflexes, "retain as habits the domination of mature behavior". This list of prepotent habits includes all the commonly recognized instincts and also a selection of admittedly learned activities such as talking, reading, writing and the learning of a trade or profession. To speak of reading or writing as "prepotent" evidently carries us a long way from Sherrington. The prepotent habit is, apparently, an activity that directly or indirectly satisfies some need or demand of the individual. But since all activities do this to some extent, the insertion of the word "prepotent" at frequent intervals does not serve to distinguish prepotent from nonprepotent activities, but rather to remind the reader that habits are not mere habits, being rooted in needs and propensities, and ultimately in native propensities.

That such is the author's intention can be judged from the list of reflexes selected by him as deserving to be named "prepotent". "A certain group of reflexes . . . were selected as the origins for which we were searching. These reflexes are classified as (1) the avoiding reactions, such as infantile withdrawing, rejecting, and struggling, and (2) the approaching responses to the stimulations of hunger and of the sensitive and erogenous zones. In the competition with other reflexes for the final common path, these reflexes are prepotent. They are of the highest importance for the welfare of both the individual and the species." "Our study of the learning process shows the necessity of regarding the preponent reflexes not as unitary pathways, but as large groups of reflexes, often diffuse in character, and involving the unspecialized play of numerous effectors. Out of this chaotic mass-response the learning process selects and fixes those movements which, in removing the source of prepotent stimulation, satisfy the demands of life." His classes of prepotent reflexes

correspond to certain fundamental needs of the organism, and his psychology is based upon needs rather than upon reflexes. He likes to connect the fundamental needs with the autonomic system: avoidance with the sympathetic division, hunger with the cranial, sex with the sacral. But he is not bound altogether to the autonomic dogma, for among the positive needs he includes, along with hunger and sex, a group of "sensitive zone responses", to serve as the basis for love of the pre-adolescent type, and also for humor and laughter.

The emphasis on the sensitive zones as an important source of pleasure and of positive or approaching behavior is original with the author. The sensitive zones are those that are ticklish, such as the lips, neck, axillae, region of the lower ribs, thigh, and soles of the feet. Light stimulation of these areas gives laughter and withdrawing movements, which, however, the author regards as "pseudo-withdrawing in type", and usually classifies among the approaching reactions. He apparently (p. 68) regards itch, as well as tickle, as predominantly agreeable, though it is safe to say that most persons avoid both sensations as far as possible.

In his genetic account of laughter, the author seizes on the undoubted fact that tickle arouses laughter in babies, and bases all laughter on that primary reaction. He shows how laughter can become "conditioned" upon the behavior of adults who are tickling the child, so that various sorts of playful attack come to serve as laughter stimuli. He does not consider the sad case of babies whose parents neglect to tickle them and thus deprive them of the opportunity of conditioning laughter to social stimuli; nor does he take account of other apparently original stimuli to laughter, as when the little child drops a spoon and bursts into laughter when it bangs on the floor. The author's account of laughter is not entirely self-consistent. In his first statement on the matter (p. 67), he regards laughter as simply a reflex response to any light stimulation of the sensitive zones; but later (p. 257), in the course of a more elaborate theory of laughter, he intimates that only tickling by other persons arouses this response. It arises only from the complex situation of a sudden, playful attack. "Laughter is uniquely a response to social situations. . . . In the original tickle situation it was always a *person* who tickled us. . . . An element of caprice, or unaccountability, peculiar to the human humor-object alone, affords the necessary suddenness for our shift of attitude"—our shift, that is to say, from fear of the apparent attack made upon us to realization that it is only a playful attack. In view of the obvious fact that the sensitive zones are continually subjected to light stimuli which do not arouse laughter, some such modification of the author's first and simpler theory is evidently demanded. Yet, on the other hand, the more elaborate theory leaves us with laughter as a native response which is itself complex, involving a shift of mental attitude, and which can be aroused only by a complex social situation. Laughter, then, would be an "instinct" in the sense which the author fundamentally rejects. All in all, the author's derivation of humor and mirth from

sensitive zone stimulation, while containing many considerations of value, is unconvincing, and does not strongly support his conception of the sensitive zones as an important source of human pleasure and behavior.

The derivation of nonsexual love from the pleasure of sensitive zone stimulation is less elaborately worked out by the author, and is even less convincing. "The caressing which children commonly receive and solicit is intimately associated with sensitive zone stimulation. Their cuddling of dolls and toys, and expression of love toward these objects, have their root in the same source." Two objections immediately present themselves. The sensitive zones are no more concerned in cuddling and embracing than are other cutaneous surfaces; and the tactile stimulation which occurs is too strong to produce tickle. A hug, or even a kiss, of any fervor does not tickle either party. If the pleasure of these contacts lies ultimately in the cutaneous sensations received, it must lie in sensations of pressure and warmth, and not in tickle. Thus the special importance of the sensitive zones in relation to love disappears. The basis of sensory pleasure and approaching reactions needs to be broadened to include the whole cutaneous surface, and, undoubtedly, sight, smell and hearing as well. Such a revision, however, would leave us with a very different basis for human behavior than that propounded by the author, with its sharply limited list of "prepotent reflexes". The situation seems to be as follows: the author started with the view of the Freudians, or especially of Kempf, that the ultimate motive forces of behavior were hunger, sex, and avoidance of injury, corresponding to the three divisions of the autonomic. Not being convinced, however, that all love and mirth were sexual in origin, the author discerned in the love of being tickled another native propensity which would broaden his foundation. It was regrettable that this additional source of pleasure lay on the surface of the body instead of in the interior, and that it seemed to have no special connection with the venerated autonomic system; but the author was too little of a dogmatist to let such considerations block his path to the truth. However, once liberated from the viscera, we discover a great variety of pleasurable external stimulations, to which we make approaching responses. Hunger, sex and tickle lose their status as sole determiners of positive behavior; the hard lines of the old scheme grow blurred, and we are left with quite a different conception, though not as yet a very clear conception.

There are one or two passages in the book which seem to recognize a love for activity for its own sake. "Baby talk", for example, is said (p. 182) to be "spontaneous and indicative of a pleasant mood. It is a form of play, a part of the diffuse outflow of energy." Originating thus in play, speech is later, by conditioning, "brought into the service of the prepotent needs" (p. 197). This derivation, which could be applied also to manipulation and to exploration by looking, listening, etc., appears to the reviewer much more sound than the alternative derivation which the author also, and inconsistently, puts forward, according to which every form of human activity is "based ultimately upon some prepotent need"

(p. 197 and passim). If play activity is recognized as spontaneous and undriven by prepotent needs; and if at the same time it is recognized as the original source of speech and of observational and manipulative dealing with objects, then it is to play rather than to the prepotent needs that we have to trace what is characteristically human in behavior. The prepotent needs enter the story, not as the great source of human activities, but as making use of them, turning them from play into work, and putting more motive power behind them. The author would probably not accept this view; but, in admitting "sensitive zone responses" as an original source of behavior, he certainly breaks loose from the prepotent needs of the organism; and, in incidentally recognizing play, he goes still further in the same direction.

If the author is to base all behavior on prepotent needs and reflexes, the question arises by what right "a certain group of reflexes was selected". By what right was hunger included and thirst excluded from the list of prepotent needs? By what right were breathing and waste elimination excluded? On what ground were fatigue and sleep omitted? Subtract rest and sleep, and human life would be radically altered. Eliminate the bedrooms of a town, and the town would be a very different affair. Eliminate the periodically recurring need of sleep, and the home would not exist as we know it. The author's plan of first selecting a few needs and building all behavior upon them is not really genetic, though it certainly has its value as a basis for discussion.

Undoubtedly the author's restricted list of prepotent needs is in line with his general desire to keep down the number of his fundamental assumptions, and in particular to minimize the scope of the native factor in behavior. Nowhere does this tendency show itself more clearly than in his treatment of the psychology of sex. First, let it be said that the author's full treatment of this matter is probably, all in all, the best that has appeared, the most matter-of-fact, scientific, and helpful. It is gratifying to find an author who can speak of sex matters without a snicker or a sneer, and who has eyes for something besides the pathology of sex life.

His general premises commit the author to a rejection of the sex instinct (as well as of the mother instinct), and to a limitation of the native factor to a few crude, simple reflexes. As usual, he is ready with a carefully wrought theory. A chain of reflexes starts with the production of the sex secretions, which then (in the male, the process in the female not being so clearly indicated), by distending the seminal vesicles, stimulate internal sensory nerves, and evoke random seeking movements—just as hunger pangs arouse seeking movements. Tumescence of the external genitals also occurs, adding a further stimulus to the seeking movements, and also serving to direct the movements sufficiently to produce contact of these organs with some object, no matter what object. This contact, acting as a stimulus, arouses the further and consummatory response of ejaculation. This is the sum total of native sex behavior, everything fur-

ther is acquired. The attraction of one sex for the other is acquired; all the adaptations of one sex to the other (except anatomical) are acquired. There is no specific native responsiveness of one sex to the other.

When a male and a female individual, each engaged in the random seeking movements resulting from the internal sex excitation, chance to come together and obtain sexual satisfaction, the reflexes of each become conditioned reflexes; the other party becomes a substitute stimulus capable in the future of arousing both the internal and the external processes already mentioned, as well as the feeling of excitement and attraction. The human adolescent does not seek satisfaction wholly at random, since social tradition steers him or her towards the opposite sex. We have to remember also that a parent is apt to have a masked sex desire for a child of the opposite sex, and that the pre-adolescent, nonsexual affection of the child is thus likely to be drawn towards the parent of the opposite sex. This prepares the youth for affection towards some substitute member of the opposite sex, provided the parent fixation can be laid aside. But all this sex polarization, in the author's view, depends on social tradition and on individual learning, and not at all upon instinct.

There are several points in this account that invite discussion.

1. The reality of the internal stimulus supposed to initiate the whole process is open to doubt. The author himself is dubious about it in the case of the female. In the male, we have no sensation analogous to hunger pangs that would indicate the existence of an internal stimulus. Nor, I believe, is it possible to point out the random seeking movements that are supposed to result directly from the internal stimulus, antecedent to any sexual experience. We know that sex secretions act as hormones, and it is quite probable that they act only thus and not as sensory stimuli.

2. The relation of erection to the supposed internal stimulus is not clear. It would be in accordance with the general trend of the author's theory if he made erection a reflex response to the internal stimulus, but he seems intentionally to avoid making a definite statement to this effect. Distension of the seminal vesicles could, indeed, not be seriously put forward as a sufficient stimulus for erection, since that would mean a continual erection in a male animal internally ready for sex activity—a condition which certainly does not represent the rule. Local or external stimuli are necessary in addition to the hypothetical internal stimulus. The "sufficiently directed" movements of a young male animal during erection can perfectly well be regarded as responses to stimulation of the erogenous zones, without lugging in the assumed internal stimulus.

3. The author assumes that the distension of the seminal vesicles (and so, undoubtedly, the production of semen) becomes readily conditioned to external stimuli. He is probably forced into this assumption by the fact that a male animal (human or otherwise) often passes quickly from a quiet, serene state into one of sexual excitement on the presentation of an external stimulus. Since the lustful condition, on the author's theory, is a direct result of the accumulation of semen, the latter must be sup-

posed to be conditioned by external stimuli in order to account for the quick mobilizing of lust. But the conditioning of a metabolic process like the production of semen is not only purely hypothetical but extremely improbable.

The preceding three criticisms, if justified, do away with the assumed internal or autonomic stimulus as the initiator of all sex behavior, and transfer this rôle to local and external stimuli. They leave untouched the probable hormonic effect of the sex secretions on various organs, producing a general readiness for sex activity, which, however, would require some local or external stimulus to give rise to actual lust or to overt sex behavior.

4. More important, perhaps, is the question whether the author comes through without any native responsiveness of one sex to the other. Is the intersexual mating which is actually so prevalent in animals and men wholly the result of trial and error and of social tradition? Such a conclusion is hardly credible, as applied to animals at least; yet the author applies it to both animals and men. For evidence, he relies heavily on Whitman's observations of pigeons.

"The internal character of the original sex stimulus is clearly shown in those animals which have well-marked breeding seasons. Such seasons depend directly upon the periodic activities of the sexual glands and smooth muscle. [Hormonic activity would however be sufficient to explain periods of heat, etc.] A pigeon which is not 'in season' will evade or repel any approach made by the opposite sex. On the other hand, a male bird in the period of sex excitement will begin the usual courtship antics at once, and, in the absence of the female, will make advances to individuals of its own sex" (p. 70).

From this it would appear, however, that, though there is no visual recognition of the opposite sex, there is at least a tendency for the "random seeking movements" to be directed towards other pigeons, rather than towards any random object. This would limit the field of search considerably. But let us continue:

"Sex recognition occurs among pigeons only through behavior. An unmated ring dove, for example, when it meets a strange dove becomes excited, charges up and down bowing and cooing, and behaves aggressively toward the other bird. If the latter is a male, he behaves in the same way, and a fight is likely to ensue. If the other is a female in season for mating, she coos seductively and assumes a submissive attitude toward the male. . . . The aggressive behavior of the male dove is a kind of control which causes the strange bird to react in a manner which reveals its sex" (p. 160).

These observations, I submit, do not in the least demonstrate an absence of specific responsiveness to the other sex. They show, indeed, an absence of differential response to the mere visual appearance of the two sexes. On the other hand, they show, first, that the sexes have characteristic differences of *behavior* under the stimulus of sexual advances made to

them; and, second, that the characteristic sex behavior of one sex is a stimulus which arouses characteristic sex behavior in the other sex.

These relations of stimulus and response between the sexes come out even more clearly in the white rat, more recently and intensively studied by Stone. A young male rat, sufficiently matured for sex activity, but as yet without sexual experience, if put together with another white rat, behaves the same whether that other is a male, a female in heat, or an inactive female. He makes advances to any of them. The other rat, if a male or inactive female, repels these advances, and there the matter rests. But the female in heat responds in a different way, to which the male in turn responds with a second phase of characteristic male behavior. This in turn evokes a further female response, and this a further male response, something entirely new in the behavior of this young male. The whole series of responses, back and forth between the two, affords a really beautiful example of natural correlation, and is absolutely inconsistent with our author's denial of specific responsiveness of either sex to the other. To be sure, we now see that it is not the visual appearance of the other sex—in pigeons and rats at least—that acts as the stimulus, nor even the odor, but rather the *behavior* of the one sex under stimulation from sex behavior by the other.

Quite possibly, the same is true of human beings. We may even grant it as probable that an adolescent boy, with no sex experience and no guidance from social tradition, would be unmoved by a statue of Venus. It is more likely the "way of a woman with a man" that constitutes the stimulus to which the man responds; and vice versa. But such a conclusion does not do away with native responsiveness to the other sex, with sex attraction, or with the sex instinct. It simply sharpens our knowledge of these matters down to a finer point.

It is strictly in accord with the author's general tendencies that he denies any specific maternal instinct. Mother love he regards as essentially identical with sex love, and as arising from the same internal stimulus. Its attachment to the child is the result of circumstances, social tradition, and especially of an "extension" of the mother's "lover-like responses" from her husband and home "to a new stimulus, the child, which through its origin as well as through its immediate presence is closely connected with the beloved objects". The fact (frequency not stated) that an unmarried mother sometimes shows no mother love is interpreted as disproving any specific responsiveness of the mother to the infant; the counter-influences, fear, shame, perhaps resentment at the father, that would have to be overcome in her case for mother love to have a fair chance, are overlooked in this interpretation.

It is obnoxious to be thus persistently fault-finding when the author's treatment of sex problems is, on the whole, so judicious. Here is just a sample of his judiciousness (pp. 360, 361):

> "Why is it that the child's love tends to be fixated upon the parent *of the opposite sex?* Since there is a strong but unrecognized sexual

> component in all adult love, it is wholly natural that the father should lavish more affection upon the daughter, and that the mother should be more strongly attracted toward the son. . . . Hence the 'choice' of the opposite-sexed parent by the child really represents the parent's choice, and results from the more intense love-making which that parent bestows upon the child. . . . The fixation upon the parent of the opposite sex is due rather to the behavior of that parent toward the child than to any instinctive sex preference upon the part of the latter."

The hostile tone of the present review, however, is the consequence of an effort to put the author's extra-simple system of fundamentals to the test. In the reviewer's opinion, the system does not stand the test. It fails to give a true picture of sex life, because it bars out all native responsiveness to the opposite sex or to the little child. In other respects, also, we have found it to be weak from lack of a sufficiently broad basis.

There still remains one major criticism to present. As will be recalled, the author rejects the concept of group behavior. Since a group is composed entirely of individuals, the behavior of the group consists entirely of the behavior of the component individuals. This guiding principle serves the author well in his analysis of the crowd. But there is a certain class of group activities that he does not consider.

Two boys, between them, lift and carry a log which neither could move alone. You cannot speak of either boy as carrying half the log, in any concrete sense, for the log is not in halves. Nor can you speak of either boy as half carrying the log, for there is no such concrete fact as half carrying the log. The two boys, coördinating their efforts upon the log, perform a joint action and achieve a result which is not divisible between the component members of this elementary group. To insist that the pair of boys consists simply of the two individuals is to commit an abstraction. It leaves out the log. By acting together upon the same object, the individuals composing the group coördinate their behavior, and the total behavior consequently possesses a unity analogous to that of a group of muscles in a coördinated movement.

Similarly, when a group of people are working together upon a problem, the contributions of the several individuals towards the solution may be impossible to disentangle; at any rate, the behavior of the group has a coördinated unity due to the common object of thought upon which their individual efforts are directed.

Other similar groups are: a ball team, the crew of a racing yacht, a gang of workmen engaged on some single, definite job, a military company, an orchestra. This type of group might be named a "team". Characteristic of the team is the presence in the situation, besides the component individuals, of some common object on which all are working, and which makes coördination possible.

Now the author, in his otherwise admirable description of aggregations, has no place for the team, and nothing to say of team work. He recognizes

three types of aggregation: (a) the face-to-face group, in which "the individuals react mainly or entirely to one another"; (b) the co-working group, in which each individual is working on his own task, not in coöperation with the rest, though subject to "social facilitation" from their presence and activity; and (c) the crowd or mob, which is, indeed, a "collection of individuals who are all attending and reacting to some common object", but whose reactions can scarcely be called team work, since they are described as "being of a simple prepotent sort and accompanied by strong emotional response. . . . Dynamically, the crowd is a large-scale suggestion phenomenon. . . . Fear, lust(!), and rage appear in their naked simplicity and barbaric strength. . . . These responses are modified in the direction of brutal strength rather than that of socialization. . . . Crowds then are struggle groups of an elementary and violent character. . . . The individual in the crowd behaves just as he would behave alone, *only more so*. . . . By the similarity of human nature the individuals of the crowd are all set to react to their common object in the same manner, quite apart from any social influence. Stimulations from one another release and augment these responses; but they do not originate them". (pp. 292-299).

The description of crowd behavior from which the above quotations are drawn is one of the best pieces of psychological analysis in the whole book; but certainly it is not a description of team work. Since, then, the crowd is the only form of aggregation recognized by the author in which the activities of the component individuals are concentrated upon a common object, his system has no place for the team or for the coördination of individual behavior into group behavior. This hiatus in his system results from his primary denial of all reality to group behavior except simply as a sum of individual behavior. It appears fair, accordingly, to conclude that the extreme simplicity or parsimony demanded by the author in fundamental assumptions results in his overlooking the most important part of his subject; for certainly team work is the most significant of social phenomena.

R. S. Woodworth.

The Education of Exceptional Children. By John Lewis Horn, Ed.D. New York. The Century Company, 1924. Pp. x+343.

Dr. Horn's book is a thorough and detailed presentation of the problems of education as psychologists and educational experts recognize them to-day. His field is a broad one, and it would be impossilbe adequately to present his work in the brief space of a review. At best it is only possible to give some idea of his most striking contributions and point out some of the defects, which are few and minor ones. The scope of his book can be pictured when it is realized that he covers such subjects as the following:

A classification of children who present educational problems.
Education of exceptionally bright children.
Education of subnormal children.

Education of physically handicapped—the blind, deaf, and crippled.
Education of the temperamentally exceptional child.
The problems of truancy and incorrigibility.
Education of speech defectives.

In the presentation of these topics an enormous amount of related material has been necessarily introduced. A discussion of the psychological methods for the measurement of intelligence; a consideration of the social aspects of these educational problems; a study of the pedagogical methods now in use in sixty-eight American cities; an evaluation of various plans of administration, and discriminating criticism of present theory and practice are all included in this comprehensive treatise.

If Dr. Horn is at all weak, it is in the early chapters in which he attempts to describe and classify the different types of exceptional children. The clinician will find his listing of truants, incorrigibles and speech defectives as the only classes which by reason of temperamental deviations become educational problems far from comprehensive. Moreover those children with special disabilities (of whom the nonreaders of good intelligence are an outstanding group), and the special educational problem which they create, have also been neglected. But it would be difficult to find other omissions.

Of the discussion of special education for the blind, the deaf, the crippled and the speech defective, it is sufficient to say that these chapters present the problems and methods very adequately. The chapters on truancy and incorrigibility are also excellent, although not as conclusive, partly, as the author explains, because scientific investigation in this field is still in its early stages, and the study of these problems is far from complete. However, the most noteworthy contribution made by Dr. Horn is not in his treatment of these subjects, but in his analysis and criticism of the reorganization of pedagogical procedure on the basis of intellectual differences.

Dr. Horn outlines a five-fold classification on the basis of intelligence; namely, the prodigy, the supertypical, the typical, th esub-typical and the feebleminded, which corresponds fairly closely to Terman's divisions of very superior, superior, normal, dull normal and defective, so far as the I.Q. grouping is concerned. The definitely feeble-minded, of the type requiring permanent institutionalization, can for the most part be disregarded; the school should not be asked to care for them, and does so only pending their admission to overflowing institutions. For the high grade defectives who will eventually be capable of self-support, for the subtypical, typical and supertypical child, Dr. Horn proposes a differentiation of educational methods on the basis of certain psychological principles.

Present plans for the reorganization of the school have been more concerned with the eight grade division of the curriculum than with the

actual needs of the child, Dr. Horn critically observes. Educators have tried three ways of fitting the child into the eight grade scheme: (1) by allowing a variety in the time for completing the eighth grade course; (2) by keeping all the children within the same limit but varying the amount of work accomplished within that period; (3) by giving special attention to the weaker individuals of the group. Each of these schemes regards the eight grade system, based largely on nothing more fundamental than chronological age and calendar units, as inevitable and to be met by adjustment rather than by abolition of the system.

After criticising these plans in more detail, Dr. Horn goes on to formulate a theory for the differentiation of education upon psychological grounds. It is his thesis that the human individual functions upon four levels—reflex, instinct, habit, and reason, and he accordingly inqvires what these facts should mean to the educator. Education he concludes, begins with the process of habit formation, and is continued in a broad sense by the transmission of social habits in the fields of religion, ethics, and group customs in general. The education of the most advanced type, which leads to discovery, invention, adaptation and progress, is dependent on reason.

Since reason cannot be utilized in the education of individuals of low intelligence, Dr. Horn proposes to make theirs primarily one of training in good habits. This is in harmony with the experience of Gesell, Fernald and Bernstein, as to the education of the high grade defective for self-support in the community. As intelligence increases, reason may more and more be taken into account as an educational method, until it predominates for the typical and supertypical group.

In the education of the prodigy, or superior and gifted child, when any provision is made at all, it is usually for rapid advancement. Dr. Horn shows the weakness of this adjustment, which does not supply the gifted child with any wider scope for his exceptional ability than the ordinary curriculum. He suggests that the time-saving idea should be replaced by one of time-consuming, in which the gifted child be given a breadth and depth of education which less fortunate children could not achieve. Enrichment of curriculum by additional subject matter, development of interests and abilities in every line, the full occupation of the early school years, so that High School and College entrance will be postponed until the child is sufficiently mature emotionally, is Dr. Horn's plan, a scheme in line with the most progressive thinking of the day.

It would be interesting to discuss these features of Dr. Horn's contribution more elaborately, but as was stated in the beginning, it is in no way possible to touch on more than the high lights of so extensive a presentation as "The Education of Exceptional Children." If the reader's curiosity is sufficiently whetted to make him study the volume itself, the purpose of this review will have been achieved. For it is

well worth perusal, both for the exactness of its information and its suggestions as to new and better ways of special education.

PHYLLIS BLANCHARD.

Child Guidance Clinic No. 1, National Committee for Mental Hygiene.

THE PSYCHOLOGY OF ACCIDENTS AND "SYMPTOMATIC ACTIONS." By Samuel A. Tannenbaum, M.D. New York. The Tenny Press, 1924. Pp. 92. Price $1.00.

In this little volume which is reprinted with corrections and additions from *Psyche and Eros,* Dr. Tannenbaum examines sixteen instances from Freud's writings in order to show that "at least as far as 'the psychopathology of everyday life' is concerned, not a single one of Freud's theories has been scientifically established, and that, in fact, they cannot be established because they are not true. The critical analysis of fourteen [the discrepancy in numbers, as Dr. Tannenbaum has culled sixteen, not fourteen illustrations, is something that needs explanation too] conspicuous examples analyzed by Freud or endorsed by him proves:

"1. That he has presented no evidence of the existence of such 'unconscious' mental processes as he makes the basis of his theories;

"2. That all these lapses (accidents, mistakes, etc.) can be satisfactorily and scientifically explained without the assumption of such 'unconscious' processes;

"3. That it is not true that repressed desires are responsible for all or most of the lapses ('Fehlhandlungen') of all sorts occurring in everyday life;

"4. That he has not proved the existence of a law of 'psychic determinism' and that he himself is not consistent in the application of this principle;

"5. That his 'interpretations' are wholly wilful, arbitrary and unverifiable; and

"6. That his theories and mode of thought are not at all scientific but mystical."

Barring the animus against Freud shown in this booklet and the belittlement of a man who is recognized even among his violent opponents as an original thinker and one who has made a lasting contribution to the mental sciences, orthodox psychologists must welcome this analysis as a keen and sound critique. It is true the author is not always fair to Freud, for example, when he makes him out to be superstitious (p. 58) because the Viennese psychoanalyst spoke of the accidental breaking of an Egyptian figure as a petitionary sacrifice to avert evil. Such *argumenta ad hominem* will not bring desirable results; but the retracing of the Freudian reasoning steps in explaining symptomatic actions has been done skilfully by the author. It is questionable whether he is justified in concluding that with the destructive analysis of the sixteen examples, the whole structure built up in Freud's *Psychopathology of*

Everyday Life falls to the ground. We must remember that besides symptomatic acts and erroneously carried out actions, Freud discusses in that book lapses of memory, lapses of speech and slips of the pen, and other such occurrences. Perhaps I may be allowed to mention in this connection that, some years ago, I subjected all the explanations of slips of the pen I could unearth in the Freudian literature to a rigorous examination and found them wanting. (*American Journal of Psychology*, 1919.) In many cases these slips demanded explanations just the reverse to those given.

A system cannot be overthrown by a partial refutation. The Freudians have just as much right to claim that the selected instances are not a fair sampling as the author is entitled to his belief that they are typical. It is only by an attack at all points that a decisive victory can be gained; and in spite of the many critical estimates of psychoanalysis in its various applications, there is still room for a comprehensive volume examining each department of this vast system separately.

Dr. Tannenbaum's brief *exposé* is purely empirical. It needs to be amplified by a broader background—a discussion of the general principles involved. His treatment is an excellent exercise in common sense logic. Freudians will regard it as both narrow and flippant, and will refuse to be confounded by the author's ruthless cross-examining. There is no doubt, however, that for detailed dissection in a circumscribed area, Dr. Tannenbaum's critique will commend itself to all who find it difficult to follow the Viennese high priest in all his gyrations.

A. A. Roback.

Harvard University.

The Mastery of Fear. By William S. Walsh, M.D. New York. E. P. Dutton and Co., 1924. Pp. xi+315. Price $2.00.

Doctor Walsh is a practical and popular minded physician who finds the manifestations of fear in almost every one on almost every occasion. He sets forth some of the foibles of fears and suggestions for "getting them under control" in this book. Unfortunately he is not as scientific and up-to-date minded as he is practical and popular.

Watson, Cannon, and the psychoanalysts receive scarcely a small paragraph together, but twenty-five pages are devoted to sex hygiene and another half dozen to constipation. Aside from looking after one's habits of sex and defecation all that is left to master fear are "measures that strengthen the will."

This resembles the one horse shay logically as well as in age but not in worthiness of construction or materials.

Donald A. Laird.

Colgate University, Hamilton, New York.

NEW BOOKS RECEIVED

(This notice does not preclude the book from further review or comment.)

The Character of Races. By Ellsworth Huntington. Charles Scribner's Sons, New York, 1924. Pp. xvi+393.

Neurological Foundations of Animal Behavior. By C. Judson Herrick. Henry Holt & Co., New York, 1924. Pp. xii+334.

Physiological Foundations of Behavior. By Charles M. Child. Henry Holt & Co., New York, 1924. Pp. xiii+330.

Primitive Religion. By Robert H. Lowie, Ph.D. Boni & Liveright, New York, 1924. Pp. xix+346. Price $3.50.

La Psychologie des Névroses. By Dr. O. L. Forel. Librarie Kundig, Geneva, 1925. Pp. 258.

The Scientific Study of Human Society. By Franklin H. Giddings. University of North Carolina Press, Chapel Hill, N. C. Pp. vi+247. Price $2.

Peculiarities of Behavior (2 vols.). By Wilhelm Stekel. Boni & Liveright, New York, 1924. Pp. xiv+328 and x+341. Price $8.50.

Telepathy and Clairvoyance. By Rudolf Tischner. Harcourt, Brace & Co., New York, 1925. Pp. xi+227.

Nature and Development of Learning Capacity. Educational Monograph Series, No. 25. By William Henry Pyle. Warwick & York, Inc., Baltimore, Md. Pp. 122.

The Psychology of the Preschool Child. By Bird T. Baldwin, Ph. D. and Lorel I. Stecher, Ph.D. D. Appleton & Co., New York. Pp. vii+305. Price $2.

Christian Missions and Oriental Civilizations—A Study in Culture Contrast. By Maurice T. Price, Ph.D. Privately printed by G. E. Steehert & Co., New York. Pp. xxvi+578. Price $3.75.

The Psychology of Emotion. By John T. MacCurdy, M.D. Harcourt, Brace & Co., New York, 1925. Pp. xvi+589.

Psychology of Religious Mysticism. By James H. Leuba. Harcourt, Brace & Co., New York, 1925. Pp. xii+336.

The Challenge of Childhood. By Ira S. Wile, M.S., M.D. Thomas Seltzer, New York, 1925. Pp. x+305. Price $3.50.

Directory of American Psychological Periodicals

American Journal of Psychology — Ithaca, N. Y.: Morrill Hall. Subscription $6.50. 600 pages annually. Edited by E. B. Titchener. Quarterly. General and experimental psychology. Founded 1887.

The Pedagogical Seminary and Journal of Genetic Psychology — Worcester, Mass.: Clark University. Subscription $5. 520 pages annually. Edited by Carl Murchison and coöperating board. Quarterly. Child behavior and genetic psychology.

Psychological Review — Princeton, N. J.: Psychological Review Company. Subscription $4.25. 480 pages annually. Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin — Princeton, N. J.: Psychological Review Company. Subscription $5. 720 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Samuel W. Fernberger.

Psychological Monographs — Princeton, N. J.: Psychological Review Company. Subscription $5.50 per vol. 500 pp. Founded 1895. Ed. by Shepherd I. Franz. Published without fixed dates, each issue one or more researches.

Psychological Index — Princeton, N. J.: Psychological Review Company. Subscription $1.50. 200 pp. Founded 1895. Edited by Madison Bentley. An annual bibliography of psychological literature.

Journal of Philosophy — New York; Sub-Station 84. Subscription $4. 728 pages per volume. Founded 1904. Bi-weekly. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology — Sub-Station 84, N. Y.: Archives of Psychology. Subscription $5. 500 pp. ann. Founded 1906. Ed. by R. S. Woodworth. Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology and Social Psychology—Boyd Printing Co., Inc., Albany, N. Y. Subscription $5. Edited by Morton Prince and Floyd H. Allport. Quarterly. 432 pages annually. Founded 1906. Abnormal and social.

Psychological Clinic — Philadelphia: Psychological Clinic Press. Subscription $2.50. 288 pages. Ed. by Lightner Witmer. Founded 1907. Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.

Training School Bulletin — Vineland, N. J.: The Training School. Subscription $1. 160 pp. ann. Ed. by E. R. Johnstone. Founded 1904. Monthly (10 numbers). Psychology and training of defectives.

Comparative Psychology Monographs — Baltimore: Williams & Wilkins Co. Subscription $5. 500 pages per volume. Edited by W. S. Hunter. Published without fixed dates, each number a single research.

Psychoanalytic Review — Washington, D. C.: 3617 10th St., N. W. Subscription $6. 500 pages annually. Psychoanalysis. Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology — Princeton, N. J.: Psychological Review Company. 480 pages annually. Experimental. Subscription $4.25. Founded 1916. Bi-monthly. Ed. by John B. Watson.

Journal of Applied Psychology — Worcester, Mass. Florence Chandler. Subscription $4. 400 pages annually. Founded 1917. Quarterly. Edited by James P. Porter and William F. Book.

Journal of Comparative Psychology — Baltimore: Williams & Wilkins Co. Subscription $5. 500 pages annually. Founded 1921. Bi-monthly. Edited by Knight Dunlap and Robert M. Yerkes.

(P. S.)